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AND

JOURNAL OF SCIENCE.

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SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L. & E. &c.
SIR ROBERT KANE, M.D., F.R.S., M.R.I.A.
WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.
JOHN TYNDALL, F.R.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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CONTENTS OF VOL. XVII.

(FOURTH SERIES.)

NUMBER CXI.—JANUARY 1859.

Page

Prof. Petzval on the Camera Obscura..... 1

The Rev. S. Haughton on some Rocks and Minerals from Central India, including two new Species, *Hislopite* and *Hunterite* 16

Prof. Challis on the Central Motion of an Elastic Fluid; and on the Theory of Tartini's Beats 21

Mr. J. N. Hearder on the Atlantic Cable 27

Dr. Heddle's List of the Pseudomorphic Minerals found in Scotland 42

Mr. T. Belt's Inquiry into the Origin of Whirlwinds 47

Biographical Notice of the late Richard Taylor, F.L.S.&c. .. 53

Proceedings of the Royal Society:—

Sir Charles Lyell on the Formation of Continuous Tabular Masses of Stony Lava on steep slopes..... 56

Dr. Joule on some Thermo-dynamic Properties of Solids.. 61

Prof. W. Thomson on the Thermal Effect of drawing out a Film of Liquid..... 61

Dr. Hofmann on Sulphocyanide of Phenyle..... 63

Dr. Hofmann on the Action of Bibromide of Ethylene upon Aniline 66

The Rev. G. Salmon on Curves of the Third Order 71

Proceedings of the Geological Society:—

Mr. J. Miller on the Succession of Rocks in the Northern Highlands 72

Sir R. I. Murchison on the Geological Structure of the North of Scotland..... 72

Prof. Huxley on the *Stagonolepis Robertsoni* of the Elgin Sandstones; and on the Foot-marks in the Sandstones of Cummingstone 75

Mr. S. H. Beckles on Fossil Foot-prints in the Old Red Sandstone at Cummingstone 77

Thoughts on the Formation of the Tail of a Comet, by J. J. Waterston, Esq..... 78

On the Difference presented by the Prismatic Spectrum of the Electric Light *in vacuo* at the Positive and Negative Poles, by Professor Dove..... 79

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NUMBER CXII.—FEBRUARY.

Page

Prof. Clausius on the Mean Length of the Paths described by the separate Molecules of Gaseous Bodies on the occurrence of Molecular Motion; together with some other Remarks upon the Mechanical Theory of Heat 81
Prof. Tyndall on Ice and Glaciers 91
Prof. Trowbridge on Deep-sea Explorations 97
Prof. Challis on the Direction of the Vibrations of a Polarized Ray 102
Mr. G. Gore on the Rotation of Metallic Spheres by Electricity. 107
Messrs. Quet and Seguin on the Stratifications of the Electric Light 109
Prof. Challis's Proof that every Equation has as many Roots as it has Dimensions 112
Messrs F. C. Calvert and R. Johnson on the Hardness of Metals and Alloys 114
Dr. Wallace on Iodo-arsenious Acid 122
Mr. A. Cayley on Poinsot's four new Regular Solids 123
Messrs. H. Sainte-Claire Deville and H. Caron on Apatite, Wagnerite, and some artificial species of Metallic Phosphates. 128
Proceedings of the Royal Society:—
Dr. Hofmann on the Action of Bichloride of Carbon on Aniline 131
Dr. Hofmann's Researches on the Phosphorus-Bases 133
Dr. Hofmann on the Action of Bisulphide of Carbon on Triethylphosphine 136
Dr. Hofmann on the History of the Monamines 138
Dr. Pavy on the alleged Sugar-forming Function of the Liver 142
Dr. Marcet on the Action of Bile upon Fats; with additional Observations on Excretine 145
Proceedings of the Geological Society:—
Dr. Dawson on Fossil Plants from the Devonian Rocks of Gaspé, Canada 147
Mr. T. S. Hunt on some Points in Chemical Geology .. 148
Mr. H. Rosales on the Gold-field of Ballaarat, Victoria .. 149
Mr. J. Harley on a New Species of Cephalaspis (*C. Asterolepis*) from the Old Red Sandstone of the neighbourhood of Ludlow 150
New Method of Examining and Verifying the Specific Gravity of Bodies, by M. A. Meyer 150
Note on a Theorem in Spherical Trigonometry, by A. Cayley. 151
Description of the Methods used to ascertain the Figure of Optical Surfaces, by M. L. Foucault 151
On some large Solar Spots, by W. R. Dawes 152

Digitized by Google

NUMBER CXIII.—MARCH.

Page
Mr. J. P. Harrison on Lunar Influence on Temperature as connected with Serenity of the Sky. (With a Plate.) 153
Prof. Faraday on Regelation, and on the Conservation of Force. 162
Mr. A. Gages on a Method of Observation applied to the study of some Metamorphic Rocks; and on some Molecular Changes exhibited by the action of Acids upon them 169
The Astronomer Royal's Remarks on Mr. Cayley's Trigonometrical Theorem, and on Professor Challis's Proof that Equations have as many Roots, &c. 176
Mr. W. R. Grove on the Reflexion and Inflexion of Light by Incandescent Surfaces 177
Prof. Hennessy on Terrestrial Climate as influenced by the Distribution of Land and Water at different Geological Epochs. 181
Dr. Simpson on a Compound of Dibromallylammonia and Chloride of Mercury 194
Dr. Simpson on the Action of Chloride of Acetyle on Aldehyde. 195
Prof. Forbes's Remarks on a paper "On Ice and Glaciers" in the last Number of the Philosophical Magazine 197
Prof. Challis on a Mathematical Theory of Heat 202
Mr. A. Cayley on Poinsot's four new Regular Polyhedra 209
Proceedings of the Royal Society:—
Dr. Debus on the Action of Ammonia on Glyoxal 210
Mr. G. B. Buckton on the Organo-metallic Radicals 212
Dr. Herapath on the Cinchona Alkaloids 218
Dr. Gladstone and the Rev. T. P. Dale on the Influence of Temperature on the Refraction of Light 222
Mr. C. Archer on the Adaptation of the Human Eye to varying Distances 224
Mr. J. A. Wanklyn on some new Ethyle-compounds 225
Proceedings of the Geological Society:—
Mr. G. P. Scrope on the mode of Formation of Volcanic Cones and Craters 229
On the Constitution of Titaniferous Iron Ores, by Prof. Rammelsberg 231
On Guayacanite, a new Mineral Species from Chili, by Frederick Field 232

NUMBER CXIV.—APRIL.

Dr. Müller on the Thermal Effects of the Solar Spectrum .. 233
Mr. W. J. M. Rankine on the Conservation of Energy 250
Mr. T. Tate on a Method of determining the Specific Gravity of Liquids 254
The Rev. S. Haughton on the Felspar and Mica of the Granite of Canton 258
Dr. Wallace on Bromo-arsenious Acid 261

Digitized by Google

Page
Mr. J. Ball on the Veined Structure of Glaciers 263
The Rev. T. R. Robinson on the Stratification of Electric Light. 269
Dr. Atkinson's Chemical Notices from Foreign Journals..... 275
Messrs. W. H. Perkin and B. F. Duppa on the Action of Pentachloride of Phosphorus on Malic Acid 280
Prof. Challis on a Method of finding the impossible Roots of an Equation, in reply to the Astronomer Royal 283
Prof. Challis on the Theory of Elliptically-polarized Light.... 285
Proceedings of the Royal Society :—
Dr. Frankland on Sodium-ethyle and Potassium-ethyle .. 289
Messrs. J. B. Lawes and Dr. Gilbert on the Composition of Animals fed and slaughtered as Human Food..... 291
Mr. B. C. Brodie on the Formation of the Peroxides of the Radicals of the Organic Acids 301
Dr. Hofmann on the Sulphocyanide and Cyanate of Naphtyle 304
Proceedings of the Geological Society :—
Mr. E. W. Binney on the occurrence of Liassic Deposits near Carlisle 305
Mr. J. W. Salter on the Fossils of the Lingula-flags.... 306
Prof. Huxley on a new species of Dicynodon 306
Mr. R. Thornton on the Coal found by Dr. Livingstone at Tete, South Africa..... 307
The Hon. C. A. Murray on some Minerals from Persia.. 307
Mr. J. W. Tayler on the Veins of Tin-ore at Evigtok, Greenland 307
Mr. J. W. Kirkby on the Permian Chitonidæ 308
Mr. J. W. Dawson on the Vegetable Structures in Coal.. 308
On Steam-ship Propulsion..... 310
Note on the Polarization of the Light of Comets, by Sir David Brewster 311
New Apparatus for observing Atmospheric Electricity, by Prof. W. Thomson..... 312

NUMBER CXV.—MAY.

Mr. W. S. Jevons on the Semidiurnal Oscillation of the Barometer..... 313
Sir D. Brewster on the Coloured Houppes or Sectors of Haidinger 323
Archdeacon Pratt on the Thickness of the Crust of the Earth.. 327
Prof. Callan on an Induction Coil of great power in proportion to its length 332
Mr. J. N. Hearder on a New Form of Telegraph Cable intended to reduce the effects of Inductive Action..... 334
Mr. J. J. Waterston on the relation of Common and Voltaic Electricity 345

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Page

Mr. W. J. M. Rankine's Note to a Letter "On the Conservation of Energy" 347
Prof. Knoblauch on the Connexion between the Structure and the Physical Properties of Wood 348
Prof. Challis on the Resistance of the Luminiferous Medium to the Motions of Planets and Comets 352
Mr. J. Cockle on the Theory of Equations of the Fifth Degree. 356
Prof. Forbes on certain Vibrations produced by Electricity .. 358
Proceedings of the Royal Society:—
Dr. Hofmann on Phosphoretted Ureas 360
Dr. Joule on the Thermal Effects of Compressing Fluids.. 364
Mr. L. Thomas on the Nature of the Action of Fired Gunpowder 366
Dr. Hofmann on the History of the Monamines 368
Mr. P. Griess on New Nitrogenous Derivatives of the Phenyle- and Benzoyle-series 370
Proceedings of the Geological Society:—
Prof. Huxley on some Amphibian and Reptilian Remains from South Africa and Australia 373
Prof. Huxley on *Rhamphorhynchus Bucklandi* 374
Prof. Huxley on a Fossil Bird and a Fossil Cetacean from New Zealand 375
Prof. Huxley on the Dermal Armour of *Crocodilus Hastingsiæ* 375
Dr. Wright on the Subdivisions of the Inferior Oolite in the South of England 376
Prof. Owen on some Reptilian Remains from South Africa. 378
Mr. E. Hull on the South-easterly Attenuation of the Lower Secondary Rocks of England 381
On the Phosphorescence of Gases by the Action of Electricity, by E. Becquerel 383

NUMBER CXVI.—JUNE.

Dr. Gladstone on the Periods and Colours of Luminous Meteors. 385
M. Buff on the Law of Electrolytic Conduction 394
Prof. Haughton on the Thickness of the Earth's Crust 397
Mr. G. Gore on an Apparatus for examining the Electrical relations of unequally heated Mercury and Fluid Alloys in conducting Liquids. (With a Plate.) 398
Prof. Challis's Theoretical Considerations respecting the relation of Pressure to Density 401
Prof. Erman on the Structure, the Melting, and the Crystallization of Ice 405
Prof. Dove on the Stereoscopic representation of Print as it appears when viewed with both eyes through Double-refracting spar 414

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Page

Prof. Dove on the Application of the Stereoscope to distinguish Prints from Reprints, or generally Originals from Copies .. 415
Prof. Tyndall on Vibrations produced by an Electric Current.. 417
Prof. Rijke on a New Method of causing a Vibration of the Air contained in a Tube open at both ends 419
Dr. Atkinson's Chemical Notices from Foreign Journals..... 422
Notices respecting New Books:—Prof. Boole's Treatise on Differential Equations 430
Proceedings of the Royal Society:—
Mr. J. P. Gassiot on the Stratifications in Electrical Discharges, as observed in Torricellian and other Vacua.. 432
Dr. Andrews and Mr. P. G. Tait on Ozone 435
Dr. Walker on Ice Observations 437
Dr. Smith on the Phænomena of Respiration 439
M. Wartmann on the Effect of Pressure on Electric Conductibility in Metallic Wires 441
Proceedings of the Geological Society:—
Dr. Falconer on the Ossiferous Cave called 'Grotta di Maccagnone,' near Palermo 442
Baron De Zigno on the Jurassic Flora..... 443
Prof. Buckman on a group of supposed Reptilian Eggs from the Great Oolite of Cirencester 444
Prof. Phillips on some Sections of the Strata near Oxford. 444
Sir P. Egerton on the Nomenclature of the Fishes of the Old Red Sandstone 445
Dr. Anderson on the Yellow Sandstone of Dura Den and its Fossil Fishes..... 446
Note on the Stratification of the Electric Light, by MM. Quet and Seguin 447
Analysis of a Native Sulphate of Copper and Iron, by M. F. Pisani..... 449
On Prof. C. P. Smyth's Teneriffe Meteorological Observations, by S. M. Drach 450
Index..... 451

PLATES.

I. Illustrative of Mr. J. Park Harrison's Paper on Lunar Influence on Temperature as connected with Serenity of the Sky.

II. Illustrative of Mr. G. Gore's Paper on an Apparatus for examining the Electrical relations of unequally heated Mercury and Fluid Alloys in conducting Liquids.

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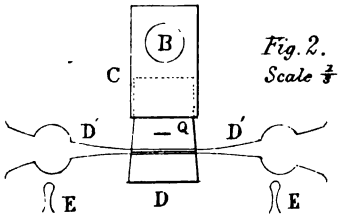


Fig. 2.
Scale $\frac{2}{3}$

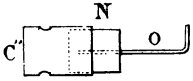
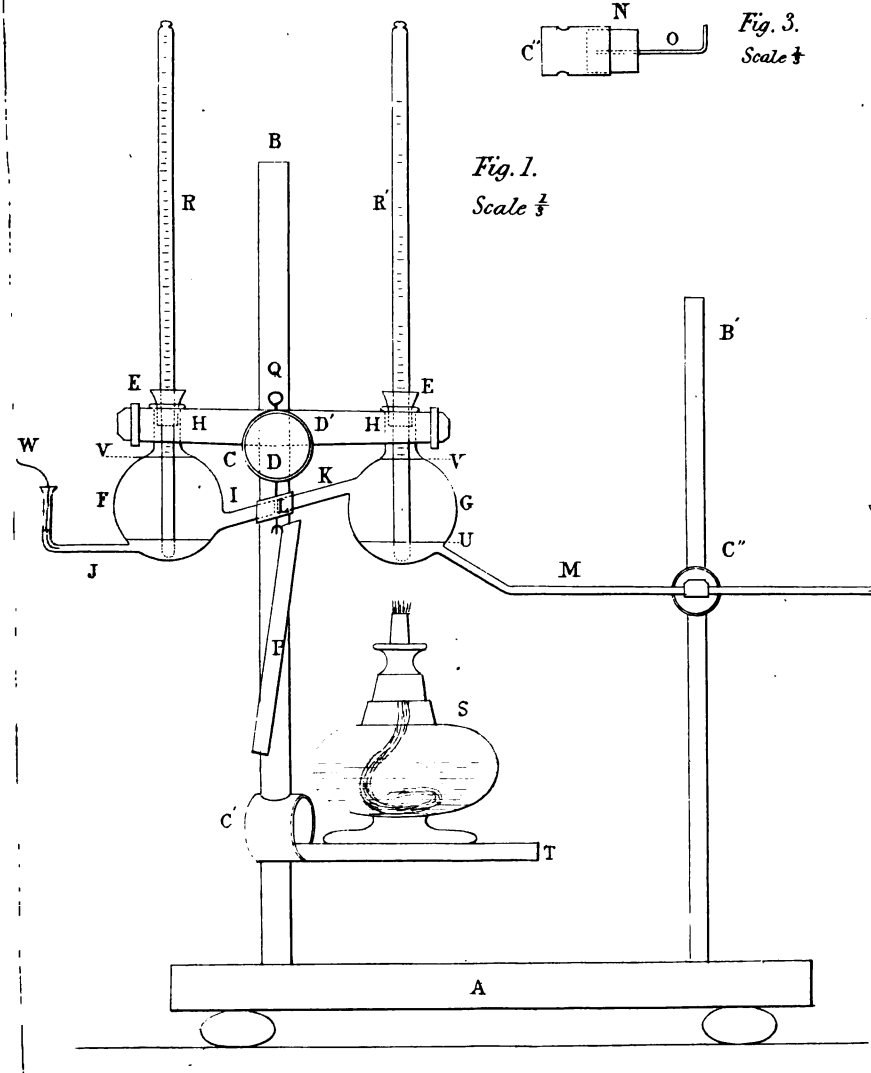


Fig. 3.
Scale $\frac{1}{3}$

Fig. 1.
Scale $\frac{1}{3}$



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[FOURTH SERIES.]

JANUARY 1859.

I. *On the Camera Obscura.* *By* Prof. PETZVAL *of Vienna**.

ON the 23rd of July, 1857, Prof. Petzval presented his new object-glass to the Academy of Vienna, and at the same time made a communication on the general properties of the camera obscura, which, from its elementary character, is well adapted to supply photographers with much accurate and valuable information. A somewhat complete abstract of this communication, therefore, cannot fail to interest a large number of readers.

A camera obscura may be defined as an instrument for obtaining, at a finite distance, an image of any number of objects; and in accordance with this definition, numerous properties at once suggest themselves as desirable. It will be well, in the first place, to enumerate the properties which may be reasonably demanded, in order subsequently to examine how far such demands can be satisfied.

We may reasonably demand that the image shall be well defined or sharp, that it shall also be well illuminated so as to exhibit proper light and shade; further, that it shall be true to nature, and also that it shall lie in a plane. If possible, too, the camera should simultaneously furnish images of both near and distant objects, should possess a large field of view, and give at pleasure either large or small pictures. Lastly, the instrument must have a convenient form, and cost as little as possible.

Most of these desiderata exist in an arrangement wherein the optician's art is quite dispensable. If a screen be placed

* For the report, of which an abstract is here given, see *Sitzungsberichte der mathem. naturw. Classe der kaiserlichen Academie der Wissenschaften*, vol. xxvi. p. 33.

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behind a small hole in the shutter of a carefully darkened room, an inverted image of external objects is at once obtained which possesses, in great perfection, many of the desired properties. We have here absolute faithfulness to nature; pictures at once of near and of distant objects, a field of vision as near to 180° as we please, and either a plane or a curved image. The expense of such an apparatus is small enough, and its convenience indisputable: in short, the picture obtained fails only in sharpness and illumination, but it must be admitted that these defects are so serious as to render the arrangement next to worthless for most purposes. Nevertheless, for many reasons, the arrangement in question deserves closer examination: it furnishes an excellent example of what nature presents, and of what art must supply; we learn from it also how often natural endowments are sacrificed when, by artificial means, we seek to enhance the nobler properties of sharpness and illumination; and lastly, we may here study the nature and influence of the imperfections inseparable from this the *natural camera.*

Let us assume that the external object is so distant, that every point of the same sends to the hole in the shutter a cone of rays so acute as not to differ essentially from a cylinder. If light were propagated in straight lines, it is manifest that the rays of every such cylinder would reach the screen in full possession of their own peculiar colour and intensity of light, and that they would impart both these qualities to a small portion of that screen, nearly circular in form, and of the same size as the hole. The several coloured spots thus formed would group themselves so as to constitute an inverted picture of the object, and the sharpness of this picture would be capable of being augmented indefinitely by diminishing the size of the hole.

Light, however, instead of being propagated in straight lines, is turned aside or diffracted on passing through an aperture, and thus gives rise to far different phænomena. The external object being a luminous point, a star for instance, its image is not only always greater than the hole, but on diminishing the size of the latter we find that, as soon as a certain limit has been reached, the image, instead of diminishing accordingly, actually becomes larger and less luminous. On closer examination this image is found to consist of a round luminous spot surrounded by concentric rings, alternately light and dark. The central spot is always found to possess the greatest intensity of light, the surrounding light rings being in general so faint as only to be perceptible by artificial means.

Since in everything which concerns the telescope, the microscope, or the camera, it is of the utmost importance to study the nature and magnitude of the defects caused in the picture by the

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diffraction of light, it will be useful to enter into further details. We may suppose the defect in sharpness to be measured by the diameter of the above circular spot, conceived to extend up to the commencement of the first dark ring. To obtain this diameter, let a right line be drawn from the centre of the hole to the screen so as to be perpendicular to the plane of the shutter. Upon this line, from the centre of the hole, set off a portion equal to the diameter of the latter. From the extremity of the line thus set off conceive another line to be drawn at right angles whose length λ is equal to that of a wave of light, *i. e.* $\frac{1}{50,000}$ in. or $\frac{1}{100,000}$ in., according as the light is red or violet. If, lastly, a line be drawn through the centre of the hole and the extremity of the last perpendicular, it will, on being produced to the screen, determine a point in the circumference of the circle, whose centre is in the line first drawn, and whose diameter, D, is required. If ρ be the radius of the hole, and A its distance from the screen, we have already

$$2\rho : A = \lambda : \frac{D}{2}, \text{ or } D = \frac{A\lambda}{\rho}. \quad . \quad . \quad . \quad . \quad (1)$$

This formula is only an approximate one, applicable when ρ is very small; in the case of a larger aperture, its diameter must be added to the value above given, that is to say,

$$D = 2\rho + \frac{A\lambda}{\rho}.$$

From the last formula we can at once deduce the best value for ρ; in other words, the size of the aperture which corresponds to the least possible value of D, and therefore to the sharpest possible image. In fact, differentiating the last expression, and setting, in the ordinary manner, $\frac{dD}{d\rho} = 0$, we find at once

$$\rho = \sqrt{\tfrac{1}{2}A\lambda}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which corresponds to

$$D = 2\sqrt{2A\lambda}.$$

For instance, if A = 11 in., then for red and violet light we have respectively

$$\rho = 0{\cdot}010 \text{ in.}, \quad \rho = 0{\cdot}007 \text{ in.},$$
$$D = 0{\cdot}042 \text{ in.}, \quad D = 0{\cdot}030 \text{ in.};$$

whence we learn that, on the whole, it would be useless to diminish the diameter of the aperture beyond $\frac{1}{60}$th of an inch, and that, under the most advantageous circumstances, the image of a luminous point is a circular spot $\frac{1}{24}$th of an inch in diameter. The picture we should obtain under these circumstances would

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clearly bear no magnifying whatever, but, on the contrary, would require to be inspected at a distance of 12 feet at least; for it is at this distance that a line $\frac{1}{24}$th of an inch in length subtends an angle of 1 minute, and it is only when objects are seen at this visual angle that they appear to be mere points. To obtain a more correct estimate, however, of the sharpness and illumination of the picture in the natural camera, let us compare it with that of a camera with a tolerably good object-glass of 3-inch aperture and 11-inch focal length. In the middle of the field the picture furnished by such a camera—there are of course far better instruments—will bear magnifying at least ten times, or, if we may use the expression, will bear inspection at a distance of $\frac{4}{3}$ths of an inch, and consequently, in point of sharpness, is $144 \div \frac{4}{5} = 180$ times superior to the picture in the natural camera. With respect to illumination, it will be observed that the two cameras have the same focal length, 11 inches, and consequently furnish equal-sized images of all external objects; their apertures, however, have the ratio 1 : 180; that of the first being $\frac{1}{60}$th of an inch, whilst that of the second is 3 inches. Now the focal length being constant, the illumination of a picture increases in proportion to the square of the aperture, so that with respect to this property, the camera with, is 32,400 times superior to that without glass. It is necessary to observe, however, that a picture so well illuminated as the one here used as a term of comparison, could in practice be scarcely obtained.

Two things are worthy of notice in the foregoing. In the first place we see how, by artificial means, that is to say, by means of well-arranged and properly curved lenses, it is possible to increase the qualities of sharpness and illumination in an instrument,—the first in the ratio of 1 : 180, and the second, indeed, in the ratio of 1 : 32,400. In the second place, we have become acquainted with a kind of aberration which puts a limit to the extreme use of diaphragms before camera lenses. To illustrate this still more, let us suppose that, in order to improve the properties of the picture, we were to try the experiment of reducing, by an interposed diaphragm, the aperture of the lens from 3 inches to $\frac{1}{2}$ an inch. It is evident from the formula (1), where $\lambda = \frac{1}{50,000}$ in., $A = 11$ in., and $\rho = \frac{1}{4}$ in., that we should thereby cause the image of a luminous point to become a round spot nearly $\frac{1}{1200}$th of an inch in diameter. Now in fine engravings, &c. we often meet with lines whose breadth is even less than $\frac{1}{600}$th of an inch; so that if our blinded lens were employed to copy such engravings, these fine lines would appear still finer in the picture, in consequence of the overlapping of the aberration circles of the adjacent luminous points. This defect would also be increased by the aberrations due to other causes, such as the

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curvature of the image, &c., so that ultimately the fine black lines of the original would in the copy be either undistinguishable, or at most mere pale shadows; at all events the picture, if it bore examination with the naked eye, would suffer no magnifying.

In order to advance step by step, let us now return to the natural camera, and seek to improve it by introducing into the hole in the shutter a small, simple, and therefore unachromatic lens of crown glass, which, for the sake of comparison, we will suppose to have a focal length of 11 inches. Let us examine what good properties are lost and gained by this certainly cheap alteration.

As long as the aperture of the lens is small in comparison with its focal length, we may safely assume that, apart from diffraction, the equally refrangible rays in any incident cylinder are made to converge to a point,—in other words, that on placing the screen properly, the image of a point in homogeneous light is itself a point. This condition of placing the screen exactly in the focus of the lens at once constitutes an inconvenience, inseparable from the new camera, which did not exist in the natural one. There are, however, graver complications to notice. Glass does not refract all rays of the spectrum alike; each differently coloured ray has a different focus, and the screen cannot of course accommodate all. To examine the consequences here involved, let n be the index of refraction for red rays, and p the distance of the corresponding focus from the centre of the lens, whose anterior and posterior surfaces we will suppose to have the radii r and r' respectively; then by a well-known formula,

$$\frac{1}{p}=(n-1)\left(\frac{1}{r}-\frac{1}{r'}\right); \quad \ldots\ldots \quad (3)$$

and $n+dn$ being the index of refraction for violet rays, whilst $p+dp$ is the corresponding focal distance, we have on differentiating (3),

$$-\frac{dp}{p^2}=\left(\frac{1}{r}-\frac{1}{r'}\right)dn=\frac{dn}{(n-1)p}$$

whence

$$dp=\frac{pdn}{1-n}.$$

For crown glass,

$$\frac{dn}{n-1}=0{\cdot}036,$$

consequently

$$dp=-0{\cdot}036\,.\,p, \text{ or } dp=-0{\cdot}396 \text{ in.} \quad . \quad . \quad (4)$$

since by hypothesis $p=11$ in.; that is to say, for the most

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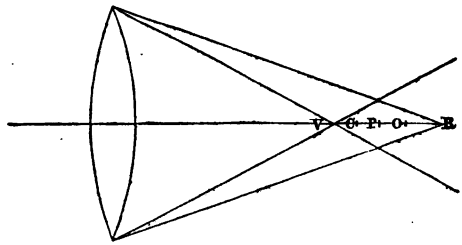
refrangible violet rays the focal length is less than that corresponding to red rays by $\frac{2}{3}$ in. nearly. If the latter combine in a point R, the former will converge to a point V nearer the lens, and the foci of all the other rays of the spectrum will lie between R and V. The rays which, on account of their number or colour,



exercise the strongest action on the retina, will be congregated about a determinate point O. Strictly speaking, the *optical focus* here referred to is not a fixed point, since all eyes are not affected in the same manner by differently-coloured rays, nevertheless it is in the neighbourhood of O that most observers obtain the best image. In another fixed point C, nearer to V than to R, will be collected the rays which exert the greatest chemical action; and at this *chemical focus*, which may also vary a little with the material exposed to the action of light, the best photographic image will be obtained. Lastly, about midway between V and R, there is a third point P at which the screen intersects the cone of rays in its narrowest part, the diameter of the circular section being

$$D = \frac{\rho \cdot dp}{p} = 0{\cdot}036\,\rho,$$

where, as before, ρ is the semi-aperture of the lens, p its focal length for red rays, and dp the so-called linear chromatic aberration*.

To this diameter of the circle of chromatic aberration must be added that of the circle of aberration due to diffraction, with which we are already acquainted, so that the total aberration will amount to

$$D = 0{\cdot}036\,\rho + \frac{p\lambda}{\rho}.$$

If we seek, as before, the value of ρ which corresponds to a mi-

* The above formula expresses an important theorem in the theory of the camera as well as in that of the telescope, inasmuch as it shows that the diameter of the smallest circle of chromatic aberration is dependent solely upon the aperture of the lens, and not upon its focal length.

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nimum of D, we readily find

$$\rho=\sqrt{\frac{p\lambda}{0{\cdot}036}},\ \text{and}\ D=0{\cdot}072\sqrt{\frac{p\lambda}{0{\cdot}036}};$$

consequently for red and violet light we have, respectively,

$$\rho=0{\cdot}08\ \text{in.},\quad \rho=0{\cdot}06\ \text{in.},$$
$$D=0{\cdot}006\ \text{in.},\ D=0{\cdot}004\ \text{in.}$$

Thus the aperture which here corresponds to the sharpest image is about $\frac{1}{8}$th of an inch, or a little more than seven times the best aperture in the natural camera. Consequently the illumination of the image is increased in the ratio of 1 : 50 nearly, though it still remains inferior to the ordinary camera in the ratio of 1 : 648. At the same time, however, the sharpness of the image has been considerably improved. In the natural camera the image of a point had a mean diameter of 0·04 of an inch; it is now diminished to 0·005. The sharpness of the image therefore is now eight times greater than before, and in this respect is only inferior to the ordinary camera in the ratio of 1 : $22\frac{1}{2}$.

These not very important improvements in sharpness and illumination have been dearly enough purchased; for although the general faithfulness to nature has not been essentially impaired, the difficulty of obtaining sharp images has been increased, on account of the chemical and optical foci being now separated by about a quarter of an inch. It is true that the difficulty here alluded to might easily be overcome if the linear chromatic aberration, dp of formula (4), and with it the distance between the foci, were always the same; for then it would suffice to place the plate destined to receive the picture a quarter of an inch in advance of the ground-glass plate. But it must not be forgotten that the formulæ (3) and (4) are true only when the incident rays are parallel, in other words, when the objects are at a great distance. An object at a finite distance a from the lens gives an image, not at the focal distance p, but at a distance α from the lens, a, p, and α being connected by the well-known formula

$$\frac{1}{\alpha}=\frac{1}{p}-\frac{1}{a}. \qquad \ldots\ldots\ldots \quad (5)$$

If we differentiate this expression according to the index of refraction, implicitly contained in p, we have

$$d\alpha=\frac{\alpha^2 dp}{p^2}, \qquad \ldots\ldots\ldots \quad (6)$$

where $d\alpha$ represents the real linear chromatic aberration, which differs from the dp of formula (4) the more, the greater the dif-

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ference between α and p, or the nearer the object. To take an extreme case, by way of example, let $\alpha=2p$, that is to say, let the distance of the object be reduced to double the focal length, then by (5) $\alpha=2p$, and by (6) $d\alpha=4dp$; so that the linear chromatic aberration is now quadrupled, and, as a consequence, the distance between the separated foci is increased to an inch. This varying distance between the chemical foci constituting, as it does, so serious a defect inseparable from all cameras with unachromatic lenses, the best possible achromatism is even more indispensable for this instrument than it is for the telescope itself.

The formula (5) also informs us of another disadvantage of the new camera as compared with the natural one. In the latter, the fact of the objects being at different distances was of no importance; in the former, however, the images of near objects are more distant from the lens than are those of more remote objects; and since the plane of the screen cannot accommodate all, it follows that if some images are sharp, others cannot be so. This inconvenience compels the photographer to have recourse to many expedients (such as grouping of the objects, &c.), of which some will be considered in the sequel.

Again, the sharpest parts of the picture of a distant plane object no longer fall in a plane, but on a spherical surface whose radius is $\frac{3}{2}p=16\frac{1}{2}$ in., and whose concavity is turned towards the lens. In consequence of this unavoidable circumstance, and the many difficulties attendant upon photographing on curved surfaces, sharpness must be sacrificed the more the field of view is increased.

Above all other things, however, the restoration of achromatism is the most important; for the chromatic aberration disappearing thereby, aperture and consequently illumination may be increased, whilst at the same time the aberration arising from diffraction will be proportionally diminished. As is well known, this achromatism is obtained by a combination of crown- and flint-glass lenses; and the method which has long been employed in telescopes not only leads to achromatism, but also diminishes a new defect known as spherical aberration. The latter manifests itself the more the greater the aperture, and is caused by the spherical form given to the surfaces of the lenses,—a form which, although most easily constructed, is not the one best adapted for causing all the rays of a pencil to converge to the same point. In ordinary telescopes the construction of the object-lens is rarely based upon strict calculations as to the best curvatures for obtaining the desired effects, the latter being determined generally by trials. The crown-glass lens is biconvex, the flint-glass one plano-concave; and the two are placed together so as

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to form a plano-convex system. When employed, the convex side is turned towards the object, the plane one towards the image, and the instrument fulfils its purposes more or less perfectly according as the curvatures of the constituent lenses have been more or less happily chosen.

In Daguerre's time these telescopic object-glasses, transferred to the camera, were in general use. In all probability, too, they were at first placed in the same manner, with the convex side towards the object; but experiment must soon have shown that this disposition was not applicable. For, destined by their construction to give very sharp but very small images, spherical aberration is destroyed only near the axis of such lenses,—in consequence of which, when the field of view is larger, a great deterioration in sharpness is observed on passing from the centre towards the edges of the picture. This deterioration is increased, too, by the fact that the image, instead of being plane, as required by the camera, lies on a curved surface which approaches in form to that of a paraboloid of rotation, whose radius of curvature at the vertex is equal to $\frac{5}{2}$ of the focal length p.

In the absence of calculations founded on theory, by means of which the sharpness at the edges of the image might be increased, opticians have sought to improve the telescopic lens, so as to adapt it to the camera, by diminishing its superfluous sharpness at the centre, or rather by rendering the contrast between the centre and the edges less striking. To obtain a notion of how this may be accomplished, let the object-lens of a good telescope be unscrewed and turned so as to present its plane side to the object. By so doing, the good telescope will be converted into a very poor instrument; and in order to obtain even a tolerable image, extreme blinding of the lens must be resorted to. The reason of this is to be sought in the serious spherical aberration that has been called into existence; the rays belonging to one and the same cylinder no longer converge towards a single point, but by their successive intersections give rise to a luminous curve or caustic, whose path intersects all the planes drawn perpendicular to the axis in the vicinity of the focus, by which latter term is now meant the point to which rays parallel to, and *very near* the axis, converge. The advantage of a diaphragm before the lens is, that it can be placed so as to admit only those rays of a cylinder whose intersections correspond to that part of the caustic which is situated in the plane of the picture. In order to convert a telescopic lens into a tolerably good camera lens, the cylinder of rays which corresponds to an image near the edge of the field must be treated in the manner described, and the position of the diaphragm determined accordingly. With a lens 3 inches in aperture and a focal length of 16 inches, such

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as was in general use in the early period of daguerreotyping, the diaphragm is best placed at a distance of 3 inches before the lens, its aperture being 1 inch. The image thus obtained, although tolerably good, will not be of uniform sharpness; in the centre it will perhaps bear magnifying three times, whilst at the edges it will barely admit of examination with the naked eye. In point of sharpness, therefore, this picture is at least three times inferior to the one of the camera already used as a term of comparison. With respect to illumination, the superiority of the ordinary modern camera is still greater; for since the degrees of illumination are directly proportional to the squares of the apertures, and inversely proportional to the squares of the focal lengths, the ratio in question is

$$1^2 \times 11^2 : 3^2 \times 16^2 = 121 : 2304 \text{ or } 1 : 19 \text{ nearly.}$$

It must be noted, however, that the modern camera has four more reflecting surfaces than the old one, by which means almost one-fifth of the light is lost, and the above ratio diminished to about 1 : 16.

The substitution of an achromatic in place of an unachromatic object-glass is, beyond comparison, the most important step in the improvement of the camera; for not only have the properties of sharpness and illumination been thereby increased—the former in the ratio of 1 : 7, and the latter even in the ratio of 1 : 40,—but the serious defect of separated optical and chemical foci has been remedied. Besides this, the image has become nearly plane—a result which, it is true, might also have been obtained in the case of an unachromatic lens by means of the same method of blinding. Lastly, the field has become almost uniformly lighted; the not very broad zone of diminishing intensity of light which still exists is due to the blinding. As diaphragms often produce this defect, it will be well to examine their action more closely.

Around the centre of the lens, and with a radius of 1 inch, conceive a circle to be described: its circumference will be at the distance of half an inch from that of the lens. The diaphragm, at the distance of 3 inches, having an aperture of 1 inch, all cylinders of rays passing through the same will be entirely received by the lens, provided their axes are within or upon the circumference of the above circle, and the corresponding images will possess the maximum intensity of light. The rays of every cylinder whose axis meets the lens in the circumference of the circle are inclined to the axis of the instrument at an angle whose tangent equals $\frac{1}{3}$, and whose magnitude is therefore 18°; consequently everywhere within a field of 36° the image possesses full intensity of light. Again, only half the rays of those cylinders

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whose axes exactly graze the edge of the lens will be admitted by the latter, the entrance of the rest being prevented by the setting. These rays are inclined at an angle of 26° to the axis of the instrument, so that between 36° and 52° the intensity of light in the field will diminish from its maximum value to one-half of the same. Lastly, the lens will admit none of the rays of the cylinders whose axes meet its plane at a distance of half an inch from its edge; consequently, between 52° and 66° the intensity of light diminishes from half its normal value down to zero. Thus when uniform light is required, the field must not exceed 36°; in other words, the focal length being 16 inches, the diameter of the circular picture cannot exceed 10 inches.

Such are the properties of the instrument with which Daguerre worked when he made his beautiful discovery. At that time silver plates, coated with iodine, were alone employed; and the time of exposure required was so great—half an hour—that portrait-taking was next to impossible. Hence arose the demand for a camera lens producing greater illumination, and equal or, if possible, greater sharpness. Sooner or later practical opticians would, no doubt, have sought to improve the camera of Daguerre by substituting a convex-concave, in place of the plano-convex achromatic lens; for the former, treated in the manner above described, possesses several advantages. Science, however, stepped in with more efficient means, and Prof. Petzval, after a thorough theoretical investigation of the subject, set about constructing his first object-glass, destined principally for portrait-taking.

In so doing he was guided by the following considerations:—The object-lens of a telescope has only three conditions to fulfil: *first*, to possess a given focal length; *second*, to be achromatic; and *third*, to reduce the spherical aberration to a minimum. The first is a matter of small importance, for within certain limits the focal length may vary; the achromatism depends on the focal lengths of the constituent lenses, and the spherical aberration on the curvatures of their surfaces. The three conditions, therefore, can be fulfilled by suitably disposing of three optical elements, *i. e.* the curvatures of three surfaces.

In the camera, however, the number of these conditions is raised from three to eight, five of which have reference to a much more complete destruction of spherical aberration, two to the production of achromatism, and the eighth to the position of the focus. Instead of three, therefore, eight optical elements are requisite, the choice of which will be determined by the following considerations. Greater illumination, one of the desired improvements, can only be obtained in two ways—by enlarging the aperture and by diminishing the focal length, both which,

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however, will result from employing two converging lenses instead of one. These lenses must of course be achromatic; and by theory, in order that a good image may be produced, they must be separated from each other by a distance not less than one-third of the focal length of the lens next the object. In order to form the eight requisite elements, therefore, seven lens surfaces and one distance may be selected. By this selection the first lens need but present three surfaces to be disposed of, so that its constituents may have a common surface; the second lens, however, in order to furnish the remaining four surfaces, must have its constituents separated, even though by so doing light is lost.

In accordance with these data, Prof. Petzval calculated, and Voigtlander constructed, a new object-lens which had an aperture of $1\frac{1}{2}$ inch, and a focal length of $5\frac{1}{2}$ inches. With it portraits were taken in forty seconds; in point of illumination it was sixteen times superior to the camera of Daguerre, and its images were sharp enough to bear magnifying twenty times. The principal defects of the new camera were a curved image and limited field of view, both of which resulted from the employment of separated lenses.

With respect to the first defect, the image of a plane object was, according to theory, situated in the hollow of a paraboloid of rotation, having at its vertex a radius of curvature equal to 7 or 8 inches. In object-glasses afterwards constructed, where the aperture was increased to 3 inches, this curvature was softened to 15 inches. By sacrificing a little sharpness at the edges, too, circumstances generally furnished means of softening this curvature still more. For portraits, indeed, a camera capable of giving a plane image of a plane object would be no acquisition, inasmuch as the persons whose portraits are to be taken by no means constitute such plane objects. With a single individual, or with a group of such, the skilful photographer may always arrange the position of his subjects so that the image will fall nearly in a plane.

The second action of the separated lenses deserves closer examination. It will be at once seen that here the setting of the first lens plays the part of the former diaphragm, and modifies the admission of light to the second lens. As an example, let us take an object-glass whose two lenses are $5\frac{1}{3}$ inches apart, the aperture of each being 3 inches. Let the focal length of the first lens be 16 inches, and that of the second 24 inches. Then by means of the first lens, a cylinder of rays parallel to the axis becomes converted into a cone, whose vertex is 16 inches behind this lens; and the plane of the second lens intercepts this cone in a circle whose diameter is diminished to 2 inches; the same

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is true approximately for every other cylinder inclined to the axis of the instrument. Around the centre of the second lens, therefore, let us conceive a circle of $\frac{1}{2}$ inch radius described; its circumference will be at a distance of 1 inch from that of the lens, and it is clear that the second lens will admit all the rays of every cylinder whose inclination to the axis of the instrument is such, that the axial ray of that cylinder, after passing unrefracted through the first lens, meets the second in the circumference of the above circle. The image produced by such a cylinder, therefore, will possess the same maximum of illumination as do the central images. But the entrance of the rays of other cylinders more inclined to the axis of the instrument will be more or less impeded; and by following the method already explained in the case of Daguerre's camera, it will be found that throughout a field of $10\frac{2}{3}°$ there will be maximum light; that between this and a field of 32° the intensity of light will diminish to half its normal value; and lastly, that the whole extent of the field, beyond which is darkness, amounts to about 50°. These angles correspond on the picture to circles whose diameters are 2, 6, and 10 inches respectively.

When portraits only are to be taken, that is to say, when a correct picture of only a small portion of the object is desired, this unequal distribution of light is of no great importance. In the case of landscapes, however, it forms a serious defect, and necessitates the use of diaphragms, not only to distribute the light more uniformly, but also to diminish the influence of the unequal distances of objects, and to soften the curvature of the image. The best place for the diaphragm is exactly midway between the two lenses, and by diminishing the intensity of light to $\frac{1}{4}$th, $\frac{1}{9}$th or to $\frac{1}{16}$th of its full value, the field of equal illumination may be increased to 31°, whilst the two zones, wherein the light first diminishes to half its normal value and then to zero, may be made much narrower. On finding that the picture thus obtained was superior to that of a camera with a simple achromatic lens, the instrument, which was constructed for portraits, was employed also for landscapes; and larger pictures being desired, the original object-glass of $1\frac{1}{2}$ inch aperture was reproduced on a larger scale, the aperture being increased to 3, 4 and even 5 inches in order to obtain pictures of 14 inches diameter. Practical opticians undertook this increase in size on their own responsibility; and the necessity of applying certain corrections to the curvatures was not attended to; the consequence of which was, that the later productions of the camera were in every respect incomplete, and deteriorated by spherical aberration, double foci, and other imperfections. These efforts to increase the size of the original instrument being in other respects unpromising,

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the demand arose for a new object-glass, which, without supplanting the old one, but rather restricting the same to the use for which it was intended, should be suited to the reproduction of landscapes, maps, engravings, &c.

The modifications applied to the old instrument to fit it for its new purposes, had for their object, principally, to increase the magnitude of the field and the uniformity of its illumination, and consisted in a diminution of the distance between the two lenses, and of the aperture of the second. The object-glass, constructed carefully with a view of fulfilling all the new conditions, and submitted to the Academy of Vienna, consists, as before, of two lenses; the first has an aperture of 3 inches and the second of of 2 inches, the clear distance between the two being 1 inch. The magnitude of the picture is the same as that corresponding to a single achromatic lens of 26 inches focal length, its diameter being 20 inches; in other words, the field amounts to 42° and is uniformly lighted. This last result is due to the diminished aperture of the second lens, and has been purchased, of course, at the expense of intensity of light. The curvature of the image of a plane object is small, its radius at the vertex being about 80 inches.

With respect to the achromatism of the two lenses, it is well known that the ratio between the indices of refraction for crown- and flint-glass is not constant, but varies with the colour of the ray, and that on this accounnt the rays of all colours cannot be made to coincide, simultaneously, by any arrangement of the two kinds of glass; in other words, according to the technical expression, a certain chromatic aberration of the secondary spectrum always remains. In the telescope most attention is paid to the coincidence of the rays at the red end of the spectrum, and, without injury to the picture, a considerable aberration of the rays at the violet end may exist. These rays, however, exert the greatest chemical action, whence it happens that the object-lens of a telescope gives a less sharp photographic, than it does an optical image. On the other hand, if the opposite end of the spectrum were most attended to, the photographic picture would be improved at the expense of the optical one, and in both cases the chemical and optical foci would be separated. In constructing the new object-glass, the whole spectrum, rather than either end of the same, was regarded, and the most active chemical made to coincide, approximately, with the most active optical rays, so that, for a healthy eye, the chemical and optical foci coincide.

From the above exposition it follows that, whilst the new camera is inferior to the old in point of illumination, it far surpasses the latter in magnitude of the field, and in uniformity of

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sharpness as well as of illumination. Whilst the new camera, therefore, is best adapted for landscapes, the old one may still be used whenever a brief period of exposure is desirable, as in taking pictures of living animals*.

To suit the properties of the new object-glass, a new camera, greatly exceeding the ordinary ones in bulk, became necessary. M. Petzval submitted a design to the Academy, remarking at the same time that it was no doubt susceptible of improvement. Without entering into a detailed description of all the arrangements in this camera, it will suffice to note the principal objects aimed at. These were a diminution of the whole mass as much as possible, a division of the same into several convenient parts, avoidance of false light, an arrangement for inclining the plane of the image to the axis of the instrument, in order to accommodate the different distances of the objects, and lastly, an improved method of uncovering the iodized plate without shaking the instrument.

Whilst admitting that his attempt to attain these objects is susceptible of being improved upon, Prof. Petzval does not hesitate to pronounce his object-glass to be the best attainable with the given optical elements. In order to test its merits, he selects the most delicate of all photographic problems, that of obtaining, on a reduced scale, the copy of a map or engraving. He enters into interesting details as to the best method to be employed, the principal feature of which consists in first bending the plane of the map to be copied into a developable surface, so as to make it approach as much as possible to coincidence with the parabolic surface, which according to theory corresponds to a plane image. As an example, he takes a map 24 inches by 16 inches, and places his instrument at a distance of 13 feet, in order to obtain a copy $\frac{1}{5}$th the size of the original. Every expedient being adopted, he calculates then that all lines in the original whose breadth exceeds $\frac{1}{240}$th of an inch will be transferred to the copy.

After some remarks on obtaining enlarged copies of pictures, and a few instructions for testing the merits of an object-glass, Prof. Petzval concludes his valuable report by stating that he has confided the construction of his new object-glasses to M. C. Dietzler.

* The new camera has also the advantage of having a more invariable period of exposure, that of the old one depending greatly on the magnitude of the field. Approximately, however, these periods have the ratio of 3 : 1.

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II. *Notes on Mineralogy.*—No. VII. *On some Rocks and Minerals from Central India, including two new Species,* Hislopite *and* Hunterite. *By the* Rev. Samuel Haughton, *M.A., F.R.S., Fellow of Trinity College, and Professor of Geology in the University of Dublin**.

Mineralogical description of a series of Rocks collected near Nágpur, Central India, by the Rev. Messrs. Hislop *and* Hunter.

THE following description of some specimens of rocks of Central India, brought home by Messrs. Hislop and Hunter, and given to me by Mr. Rupert Jones, of the Geological Society of London, may prove of some interest, particularly with reference to the metamorphic action of water at a high temperature, to which the attention of English geologists has been recently called by the valuable paper lately read by Mr. Sorby before the Geological Society of London.

No. 1. Radiated concretionary nodule of brown carbonate of lime and iron; contains no magnesia. Locality: Girad.

No. 2. Dark green calcareo-micaceo-hornblendic rock; metamorphic. Locality: Jambul Ghat, South of Nágpur.

No. 3. Coarsely crystalline saccharoid dolomite, with long white crystals of Tremolite, very like some specimens from the Val Tremola, St. Gothardt. Locality: Korhádi.

Upon a chemical examination of the calcareous portion of this rock, I found it composed of—

Carbonate of lime	144 parts
Carbonate of magnesia . . .	89 „

giving an atomic proportion of—

CaO, CO^2	2·88	4
MgO, CO^2	2·12	3

This rock is therefore a pure dolomite, and resembles in its chemical composition many of the metamorphic magnesian limestones.

No. 4. *Hislopite.*—Green calc-spar. Locality: Tákli.

This is a very remarkable mineral; it presents the crystalline form of calc-spar; its colour is a brilliant grass-green; and it effervesces briskly with weak hydrochloric acid, which dissolves its calcareous portion, leaving a beautiful green siliceous skeleton, similar to those to which M. Alphonse Gages has recently directed the attention of geologists.

* Communicated by the Author, having been read before the Royal Dublin Society in November 1858, in whose Museum most of the rocks and minerals are deposited.

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I made a careful analysis of the whole rock and of its green skeleton, with the following results.—N.B. The quantity operated on was 39·31 grs.

Green Calc-spar.

	Actual weight.	Per cent.
Green siliceous skeleton . .	6·54	16·63
Alumina	0·29	0·73
Carbonate of lime	31·76	80·79
Carbonate of magnesia . .		trace
	38·59	98·15

Spec. grav. =2·645.

Upon fluxing the siliceous skeleton with carbonate of soda and potash, I found—

Green Siliceous Skeleton.

	Actual weight.		Per-centage.
Silica	3·57		54·59
Alumina	0·31		4·74
Peroxide of iron.	1·66	Protoxide .	22·84
Carbonate of lime	0·11	Lime . .	0·94
Pyrophosphate of magnesia .	0·90	Magnesia .	4·90
		Water & loss	11·99
			100·00

The mineral just described appears to me to be the *Glauconite* of the American mineralogists, described by Dana, Rogers, and others. I find its atomic proportions as follows:—

	Number of atoms.	Oxygen ratio.
Silica	1213	1213
Alumina	91	395
Protoxide of iron . .	634	
Lime	33	
Magnesia	245	
Water	1332	

From the oxygen ratio here given, I deduce the following mineralogical formula for this remarkable mineral:—

$$\left.\begin{matrix}3RO\\Al^2O^3\end{matrix}\right\}3SiO^3+3HO. \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

Or, Hydrated tersilicate of protoxide of iron.

The following analysis by Rogers, of Glauconite from the green grains of the greensand formation of New Jersey, will serve to show the probable identity of Glauconite with the green siliceous skeleton of the green calc-spar of Nágpur:—

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	Per-centage.	Atoms.	Oxygen ratio.
Silica	53·26	1183	1183
Alumina	3·85	74	
Protoxide of iron	24·15	671	
Magnesia	1·10	55	391
Lime	1·73	61	
Potash	5·36	114	
Soda	1·60	51	
Water	10·12	1124	
	101·17		

No formula has been proposed by Dana for Glauconite; but it is evident that the preceding analysis gives very exactly the formula (I.) found from Mr. Hislop's mineral. I propose to give the name of Hislopite to the remarkable combination of Calc-spar and Glauconite found by him at Nágpur: the two minerals mutually penetrate each other, the calc-spar giving a form to the whole, and constitute together a beautiful example of the mineralogical law to which attention has been directed by M. Gages.

No. 5. Doleritic lava, with amygdaloidal geodes and cavities, invariably lined with obsidian in a thin glazed pellicle, and occasionally filled up with tabular crystals of calc-spar. Locality: Sitavalli Hill.

Var. a. Dolerite, partly amygdaloidal; cavities lined with green earth, and sometimes filled with calc-spar in flat crystals. Same locality.

No. 6. Calc-spar, curiously striated; the lines of growth not being perpendicular to the optic axis, but formed by planes parallel to one of the edges of the obtuse trihedral angle of the rhombohedron, and intercepting equal portions on the other two edges of that angle. Locality: Tákli.

No. 7. Specimen of quartz rock from the Weiragad Diamond Mines. Deposited by the action of water.

No. 8. Graphic or letter-granite, composed of cream-coloured felspar and watery quartz. Locality: City of Nágpur.

No. 9. Coarsely crystalline granite, composed of—

1. Quartz of watery lustre.
2. *Hunterite.* White felspathic mineral of fatty lustre, softer than felspar, but gritty under the agate pestle.
3. Pink felspar, in large tabular crystals (1 in. by ½ in.), with brilliant reflexion.

The white felspathic mineral contained in this granite ap-

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peared to me so peculiar, that I analysed it carefully with the following results.—N.B. Quantity operated on =27·56 grs.

Fatty Felspathic Mineral.

	Actual weight.	Per-centage.
Silica	18·17 grs.	65·93
Alumina	5·78 „	20·97
Lime (carbonate) . . .	0·15 „ Lime .	0·30
Magnesia (pyrophosphate).	0·35 „ Magnesia.	0·45
Loss by ignition	3·20 „	11·61
Spec. grav. = 2·319.		99·26

The atomic proportions given by the preceding analysis are—

SiO^3	1465
Al^2O^3.	403
RO	32
HO	1290

The silica here far exceeds in amount that found in any Kaolin, and in fact the relation of silica to alumina is much greater than is requisite to form orthoclase. In its present state, the waxy felspathic mineral of this granite, if it resulted from the decomposition of orthoclase, must have done so under circumstances that rendered the restoration of the silica removed as silicate of potash, an operation that proceeded *pari passu* with the decomposition of the felspar. The mineral presents no appearance of disintegration, its edges of separation from the pink felspar are well defined; and although its fatty lustre makes the mineralogist suspect the presence of water, yet it has all the appearance of a mineral formed in the bosom of the molten granite at the time of its eruption (or deposition?). If so formed, it should not be considered as a decomposed mineral at all, but as an independent hydrated silicate of alumina, formed under great pressure and at a high temperature, water being present. In the granites of Leinster, the margarodite mica, which is present in large quantities, contains 5 or 6 per cent. of water; and yet it has never been, nor could it properly be, looked upon as the result of any metamorphic action on the rock.

I believe the hydrated aluminous silicate of Nágpur to have been formed in the granite originally, or at least that, if it be the result of subsequent metamorphic action, the latter must have taken place at a high heat, and, as water was present, under a great pressure. If the destruction of orthoclase was necessary to its formation, as fast as the silicate of potash was removed by the heated water (probably red-hot), the silica must have been replaced, perhaps at the expense of the quartz of the granite, which is very abundant; or, which comes to the same, the metamorphosing agent was highly heated water under pressure,

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holding silica in solution, as we know it to be capable of doing to a very great extent.

In whatever point of view we regard this mineral, it must be considered one of great interest, in consequence of its being a hydrated aluminous silicate, without protoxide bases, and containing a proportion of silica to alumina the same as that found in orthoclase. I am of opinion that it is a new mineral species, and I would propose to call it *Hunterite*, in honour of one of the gentlemen who brought it to England. If we neglect the lime and magnesia, it may be regarded as having the following mineralogical formula,—

$$5[Al^2O^3, 3SiO^3 + 3HO] + [HO, 3SiO^3]; \quad . \quad . \quad (II.)$$

being, in fact, composed of five atoms of a hydrated tersilicate of alumina* combined with one atom of a hyaline silica of admitted composition. It appears to me to be a confirmation of this view of the mineral, that in the gneiss that accompanies the granite of Nágpur, and is often undistinguishable from it, this fatty felspar often passes into yellow and pinkish opalescent minerals, with which it evidently has the closest relation.

If we take account of all the elements present, and adopt Scheerer's view of the replacement of magnesia by three atoms of water, we find the following, which is exact:—

$$403[Al^2O^3, 3SiO^3 + HO] + 256\left[\begin{matrix}RO\\3HO\end{matrix}\right\}SiO^3\Big] + 215HO;$$

or, approximately,

$$4[2(Al^2O^3, 3SiO^3) + 3HO] + 5\left[\begin{matrix}RO\\3HO\end{matrix}\right\}SiO^3\Big]. \quad . \quad (III.)$$

Whatever view be adopted as to the rational formula of this mineral, it is certain that part of its silica is in chemical combination with water; and if it be regarded as a metamorphic orthoclase, it is to be considered as one from which only $\frac{5}{12}$nds of the silica has been removed, and that the potash has been chemically replaced by water.

No. 10. Gneiss formed from the preceding granite, composed of—

1. Watery quartz.
2. Pink, white, and yellow opaline felspar, with a waxy lustre.

Locality: Nágpur City.

No. 11. Fine-grained granite, composed of—

1. Quartz.
2. White felspar.
3. White mica in lozenges.
4. Small crystals of garnet (abundant).

Locality: Korhádi.

* In fact, an *aluminous Glauconite*.

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No. 12. Gneiss, composed of—

1. Quartz.
2. Pink orthoclase felspar (¼ to ½ in.).
3. Epidote.

Locality: North of Segaum.

No. 13. Gneissose granite, composed of—

1. Quartz.
2. Red orthoclase felspar (predominant).
3. Blackish-green mica (abundant).
4. Epidote (traces).

Locality: Nullá, North of Segaum.

No. 14. Siliceous breccia, formed of angular pinkish-white opake jasper (¼ in.), cemented by silicate of iron, forming a delicate green jasper. Locality: Tertiary beds of Telankhedi.

No. 15. Crystalline calcareous limestone, containing strings and patches of red hæmatite; possibly of some value as an iron ore. Locality: Korhádi.

No. 16. Crystalline calcareous limestone, with traces of grossularite; probably metamorphic. Locality: Korhádi.

No. 17. Brown hæmatite, in stratified nodular concretions; like the oolitic ore of Leicestershire. Locality: Iron-band of Sillewáda.

No. 18. Black tourmaline crystals, in quartz. Locality: Nágpur City.

No. 19. Amygdaloidal basalt, decomposing; contains rounded cavities filled with green earth. Locality: Sitawaldi Hill.

III. *Questions in Hydrodynamics*:—(I.) *On the Central Motion of an Elastic Fluid.* (II.) *On the Theory of* Tartini's *Beats. By the* Rev. J. Challis, *M.A., F.R.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

I. It appears to be established by modern experimental researches, that the different physical forces are mutually related by some common condition, or bond of connexion; but what the precise nature of the connexion is, perhaps experiment alone is incapable of determining. This generalization will become matter of exact knowledge only when it is brought within the domain of mathematics. The great desideratum of the existing state of natural philosophy is a *mathematical theory of physical forces*. After the explanations that have been given of

* Communicated by the Author.

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a great variety of phænomena of light, which is one of those forces, by the hypothesis of a highly elastic medium pervading space, it is not a little surprising that an explanation of the correlation of the several forces should not have been sought for in the existence of this medium, which would seem to be a vast reservoir of force sufficient to account for all observed dynamical effects. So long since as 1840, in the Philosophical Magazine for December of that year, I called attention, with implied reference to such a general theory, to the importance of giving an answer to the following question:—If a minute spherical atom were subject to the mechanical action of the vibrations of a very elastic medium, like those which take place in air, would it, in addition to a vibratory motion, receive also a permanent motion of translation? But I know of no attempt at the solution of problems of this kind excepting one which I made in a paper communicated to the Cambridge Philosophical Society, and published in vol. vii. part 3 of their Transactions. By taking into account the square of the velocity and condensation, I have there arrived at the conclusion, that the small sphere would be permanently moved in the direction of the propagation of the vibrations. Although, if I attempted the same problem now, I should treat it in a different manner, the result, I have reason to say, would be substantially the same as to the effect of the terms of the velocity and condensation that are of the second order. I should also be prepared to indicate the circumstances under which the motion of translation might take place in the direction contrary to that of propagation. In a theory of physical forces the latter of these effects would correspond to attraction, and the former to repulsion.

It must, however, be admitted that the great obstacle to this kind of research is the very imperfect state of the mathematical theory of the motion of fluids. The principles according to which partial differential equations are to be applied, and their solutions interpreted, are far from being well understood. Having given attention to this department of applied mathematics a great many years, I am fully sensible of the difficulties which beset it, and am able to point only to a few results of my researches which appear to be satisfactorily established. Those which may be considered of chief importance rest upon the following principles:—(1) The motion of a fluid, so far as it is not arbitrarily impressed, but results from the mutual action of the parts of the fluid, obeys the law of continuity. (2) Those circumstances of the motion that are not arbitrary cannot be inferred from the *arbitrariness* of the functions which integration introduces. The first of these principles is made the foundation of a new hydrodynamical equation, which may be called the equation of the

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continuity of the motion. By using, in accordance with the second of the above principles, this general equation with the two others that have been long known, I have obtained, prior to any supposed case of disturbance, the following general results:—(1) The small vibrations of an elastic medium, of which the pressure (p) and density (ρ) are related by the equation $p=a^2\rho$, in their simplest form are parallel and perpendicular to a rectilinear axis of propagation. (2) Any number of such small vibrations may coexist. (3) The form of the function which expresses the velocity (v) and condensation (s) along the axis of a simple vibration is that of the sine of a circular arc, and the values of v and s are given by the equations

$$v=\pm\kappa as=m\sin\frac{2\pi}{\lambda}(x\mp\kappa at+c).$$

(4) To satisfy the conditions of any arbitrary small disturbance, the motion must be supposed to be composed of simple vibrations, their number, the directions of their axes, and the values of m, λ, and c being arbitrary.

These results may be at once applied in the undulatory theory of light to account for the kind of vibration which is alone found to accord with phænomena, for the composite character of light, and for polarization. Unless these prominent and general facts receive an *à priori* explanation, we can hardly be said to possess a theory of light. The success with which such explanation is given by the undulatory theory of light based on hydrodynamical principles, may be considered as some evidence that the investigation has taken a right course, and affords a presumption that the same kind of treatment may be applied to the other physical forces.

There is, however, a case of the motion of an elastic fluid the mathematics of which must be settled before any such application is possible, viz. that in which the condensation is a function of the distance from a centre, and the motion is in straight lines passing through the centre. This may be called central motion, as forces tending to or from a centre are called central forces. I long since pointed out a difficulty which presents itself in the usual treatment of this case of motion. The known partial differential equation of the second order between the condensation (s), the distance (r) from the centre, and the time (t), is satisfied by the solution,

$$as=\frac{f(at-r+c)}{r},$$

from which it necessarily follows, on the principle of the discontinuity of the function f, that a single wave of condensation of given breadth may be propagated uniformly from the centre,

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and at the same time its condensation vary inversely as the distance; whereas it is certain that in this case the condensation must vary inversely as the square of the distance in order that the quantity of the fluid may remain constant. The former inference is, however, strictly drawn according to the principles usually adopted in the application of analysis to the motion of fluids, the very same process being employed in determining the velocity of propagation of vibrations. I do not therefore see how, on those principles, the difficulty I have indicated can be overcome. What would be at once said according to the views that I take is, that all such reasoning is essentially faulty, because circumstances of the motion which are not arbitrary are thus made to depend on the arbitrariness of the functions. My reason for adverting to this subject again is, that I have recently found, contrary to what I had supposed to be the case, that the law of the variation of condensation according to the simple inverse of the distance, is in strict accordance with the views that I advocate, and consistent with the principle of the constancy of mass. These results have been arrived at in the following manner.

At the end of a communication to the Number of the Philosophical Magazine for February 1853, I have given a solution of the problem of the central motion of an elastic fluid, which, although not as general as it might be made, conforms exactly to the principles antecedently laid down. The solution proceeds on the hypothesis that the central motion results from the composition of an unlimited number of the simple vibratory motions, which, as already stated, are deducible from the hydrodynamical equations prior to any case of arbitrary disturbance, the axes of the motions passing equally in all directions through the centre. The central motion and law of condensation are thus obtained independently of any case of disturbance, and must consequently be such only as result from the mutual action of the parts of the fluid. The expressions for the velocity and condensation which this process gives are the following:—

$$v=\frac{m}{r}\cdot\left\{\cos\frac{2\pi}{\lambda}(r-\kappa at+c)+\cos\frac{2\pi}{\lambda}(r+\kappa at+c)\right\}$$
$$-\frac{m\lambda}{2\pi r^2}\cdot\left\{\sin\frac{2\pi}{\lambda}(r-\kappa at+c)+\sin\frac{2\pi}{\lambda}(r+\kappa at+c)\right\},$$
$$\kappa as=\frac{m}{r}\cdot\left\{\cos\frac{2\pi}{\lambda}(r-\kappa at+c)-\cos\frac{2\pi}{\lambda}(r+\kappa at+c)\right\}.$$

If we consider apart the terms relating to propagation from the centre, it will appear from the term $-\frac{m\lambda}{2\pi r^2}\cdot\sin\frac{2\pi}{\lambda}(r-\kappa at+c)$,

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that there is a constant flow of fluid from the rarefied parts of the waves to the condensed parts, causing the condensation and rarefaction to *decrease more slowly* with the distance than according to the law of the inverse square. On calculating the amount of fluid that thus flows, it will readily be found to be just the quantity required to change the law of decrement from the inverse square to the simple inverse of the distance. Thus the difficulty before stated is explained on the hydrodynamical principles which I have proposed.

The same communication contains the indication of another method of solving the same problem, by means of which the following more general results might be obtained:—

$$V = \Sigma . b = \frac{1}{r}\Sigma . \left\{ m \cos \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\}$$

$$= \frac{1}{r^2}\Sigma . \left\{ \frac{m\lambda}{2\pi} \sin \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\},$$

$$\kappa a S = \kappa a \Sigma . s = \frac{1}{r}\Sigma . \left\{ \pm m \cos \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\}.$$

The circumstances of any given small disturbance may be satisfied by these equations.

There is also another point relating to the central motion of an elastic fluid which requires elucidation. In the Number of the Philosophical Magazine for November 1853, I have shown that the integral of an equation which is admitted to be true, viz.

$$\frac{d\rho}{dt} + \frac{d . V\rho}{dr} + \rho V \left(\frac{1}{r} + \frac{1}{r'} \right) = 0,$$

when applied to motion propagated uniformly from a centre, proves that the condensation varies inversely as the *square* of the distance. In reliance upon this reasoning, I have assumed the law of the inverse square in the solution of several hydrodynamical problems. But I now perceive that the assumption was not allowable, because the above equation was obtained by geometrical considerations applied to the motion and density of the fluid, *prior to any consideration of its pressure*. The law of decrement of condensation which it gives depends therefore solely on constancy of mass and continuity of the motion, and admits of being modified by the action of forces. Thus this result is compatible with the law which, as we have seen, is obtained when the dynamical equation is taken into account.

II. I pass now to the theory of Tartini's beats, in order to meet an objection which might be raised against the fourth of the general hydrodynamical results before mentioned. It might be

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urged that if the small vibrations of the æther are always composite, and this property explains the composition of light, there ought to be some indication of the same property in aërial vibrations, the æther having been assumed to be constituted like air. I believe, for reasons which follow, that such indication is given by certain audible sounds, which have been named Tartini's beats, from their having been first discovered, or specially noticed, by that musician. These beats are to be distinguished from the beats of imperfect consonances, which are heard when the ratio of the periods of vibration in two series of waves is a little different from the ratio that produces harmony, whereas Tartini's beats are best heard when the concord is most perfect. This difference was well understood by Smith ('Harmonics,' Prop. XI. Schol. 3), who for distinction calls the latter "flutterings," but appears to have noticed them only in sounding together notes the ratio of whose times of vibration was expressed by numbers too high for musical harmony. When the ratio is that of two low numbers, as 3 and 5, the "flutterings" of Smith become "the grave harmonic" of Tartini. Recently, Professor De Morgan, in the Cambridge Philosophical Transactions (vol. x. part 1. p. 136), has proposed to explain the grave harmonic by a certain relation of the *phases* of the component vibrations, thus making it dependent as to degree and quality on the manner in which the aërial pulses are *started*. But Tartini and other musicians tell us that if only the condition of perfect consonance is fulfilled, the grave harmonic is always equally heard. There is, therefore, still something to account for. The explanation offered by the theory of aërial vibrations which I have advanced, is as follows. The state of the fluid as to velocity and condensation along a straight line of propagation in the positive direction, is expressed generally by the equations,

$$V = \kappa a S = \Sigma . \left\{ m \sin \frac{2\pi}{\lambda} (\kappa a t - x + \epsilon) \right\},$$

the symbol Σ embracing as many terms as we please, having different values of m, λ, and ϵ. In general, to satisfy an arbitrary disturbance, the values of λ and ϵ must not be limited as to consecutiveness. But if the disturbance be such as to produce a perfect consonance between two notes, the component vibrations will group themselves into two sets having values of λ corresponding to the notes. In each set the values of m will be very small and those of ϵ may be nearly consecutive, if, as the analogy of light-vibrations would lead us to expect, the number of the simple vibrations be very great. The resultant of each set, as is known, consists of vibrations having the same value of λ as the components; and the most marked effect on the ear from

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the composition of the two resultants is a harmony, more or less agreeable, according to the simplicity of the ratio of the values of λ. This effect, it appears, is independent of the resulting phases. But the component vibrations of each note may also be grouped according to the values of c; and assuming these values to be *quam proxime* consecutive, the number of the groups may be very numerous; and two may be selected, one from each set, such that the maxima or minima of their condensations shall be very nearly coincident at equidistant points along the line of propagation. The resultant of these two groups will stand out by this circumstance from all the other resultants of two groups; and a musical ear might detect the periodicity from its prominent character, although this resultant might not be more audible than the others. In this manner the theory accounts for a perfect consonance being accompanied by Tartini's beats, or grave harmonic, for the constancy of this occurrence, and for the small intensity of the sound. This evidence, that sound-vibrations are compounded like light-vibrations, is sufficient to meet an objection to the proposed theory of the composition of light on the ground of a supposed diversity between light and sound in this respect.

Cambridge Observatory,
December 13, 1858.

IV. *On the Atlantic Cable.*

By J. N. Hearder, *Electrician, Plymouth**.

THE suspense and anxiety occasioned by the present unsatisfactory condition of the Atlantic Telegraph Cable, have afforded opportunity for an endless amount of speculation as to the cause of failure, and the prospect of ultimate success, and for an infinite variety of propositions for new forms of cable, intended to prevent the liability to accident and remove the objectionable peculiarities of the present one. These have included some of the most ridiculous schemes that could well emanate from individuals quite unacquainted with the subject.

During the last twelve months the journals have been teeming with notices of new patents, many of them for the revival of contrivances which have long become obsolete, but which appear, nevertheless, to be quite new to the present projectors. Some have gone so far as to patent impossibilities, whilst others, better informed, have introduced some valuable improvements.

In the cure of any disease, it is generally admitted that the discovery of its cause is half the battle; and it will be the

* Read at the Plymouth Institution and Devon and Cornwall Natural History Society, December 16th, 1858. Communicated by the Author.

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object of the present paper to examine a few of the peculiarities of submarine cables in general, and the Atlantic Cable in particular, together with the electrical appliances employed for the latter, with a view of ascertaining, if possible, the suitability of the means to the end, and thence drawing some practical conclusions, which it is hoped may serve for future guidance.

In order to do this, we must take a brief view of the different conditions and functions of atmospheric and subaqueous lines, and the phænomena to which they give rise.

With a freely-insulated atmospheric wire, that is to say, with a wire suspended in the air by insulating supports, after the manner of our ordinary telegraph wires, the study of the phænomena developed in working through it is comparatively simple, and the laws easily deducible. They resolve themselves principally into the relation between the electro-motive force of the battery, and the resistance of the wire through which the current has to pass. I use the term resistance because it is a more significant term than the converse one of conducting power. It was formerly the custom to designate metals conductors of electricity; and so they are to a certain extent, but they are all relatively so, and the best conducting of them afford a certain amount of resistance. By the use of suitable instruments the relative degrees of conducting power of the various metals, or of different samples of the same metals, or, in other words, their relative resistances to the force of the electric current, can be accurately determined.

The Society will remember that in the year 1842 I exhibited a magnetometer, which I had invented for determining the relation between the electro-motive force of different voltaic arrangements, and the resistance of conducting wires under various conditions, as well as the influence which these modifications exerted over the development of magnetism in iron. For this invention I was honoured, in 1844, with the prize silver medal of the Royal Cornwall Polytechnic Society. The engraving and description of the instrument will be found in the report of that Society for 1844. In April 1845, I exhibited the instrument at the London Institution; and an account of it was given in the 'Electrical Magazine,' vol. ii. p. 133. More recently an engraving and description of it have appeared in Dr. Noad's Manual of Electricity.

The instrument is now before the Society; and I have introduced it this evening because great stress has been of late laid upon the valuable results arrived at by the employment of a magnetometer, to the invention of which Mr. Whitehouse, the electrician of the Atlantic Company, has laid claim, and with which, to use his own expression, he weighs the strength of the

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current. Mine consists of a long and delicate steelyard, which is supported between two iron pillars, about 30 inches high, firmly fixed in a heavy base-board about 4 feet long and 1 foot wide. From the short end of the lever hangs a steel hook for holding the keeper, with a contrivance for raising or depressing it. Magnets, of various kinds, can be fixed vertically on the base-board, under the keeper. One of the most useful forms of magnet is a U-shaped one about 12 inches high and $1\frac{3}{8}$ diameter. Upon its poles is coiled a rope, consisting of 24 strands of No. 16 copper wire, each 12 feet long, covered with cotton and varnished previously to twisting. The form of wire rope is preferred, as every strand bears the same relative position with regard to its power of influencing the magnet. The ends of these wires are severally connected with twenty-four pairs of binding screws, fixed in a flat piece of mahogany in front of the magnet, in such a manner as to admit of their being joined in various modes, either collaterally or consecutively. For example, they may be made to form a short conductor of 12 feet in length and 24 wires in thickness; or they may be united end to end, to form one continuous conductor, 288 feet in length, and a single wire in thickness; or they may form any intermediate length and thickness. The only difference between Mr. Whitehouse's magnetometer and mine, is that he has placed his magnet horizontal, whereas mine is vertical. I do not mean, for a moment, to imply that Mr. Whitehouse derived his ideas from my instrument; but I merely wish to state that mine was made and its construction published sixteen years since, whilst, I believe, Mr. Whitehouse's magnetometer is not yet three years old.

The mode in which I apply this instrument, to ascertain the resistance of any conducting circuit as compared with any other, is to introduce into the circuit between the voltaic battery and the magnet, a known length of wire to be tested. The amount of attractive force developed by it is then noted, the wire removed, and another wire substituted for it. The attractive power is again noticed, and should it vary, the length of the wire then in circuit is increased or diminished until the attractive force is made precisely equal to that of the first experiment. Their relative resistances, or, in other words, their relative conducting powers, are thus easily determined. For instance, supposing that the diameters of the wires are precisely equal, their relative resistances will be in the inverse proportion of their respective lengths, so that if a wire requires to be reduced to half the length of another wire of similar thickness to produce the same effect, it shows that it has only half the conducting power, or, in other words, double the resistance.

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Secondly, their lengths being equal, their relative resistances will be directly as their mass; for the wire which requires to have its thickness increased to produce an equal effect, offers the greatest resistance, and is the worst conductor. It is for this reason that when iron wires are used for telegraphic purposes they require to be very much larger than when of copper.

By means of this instrument, the Society will recollect that I determined many of the relations between the energy of certain voltaic arrangements and the conducting power of various wires under different conditions; and though it may appear strange, yet it is a fact, that the results which I obtained and detailed to the Society from fourteen to sixteen years since, would, if I were to publish them now, be quite new to the scientific world. The laws of electro-motive force and resistance have, however, been determined by instruments of a different character, such as galvanometers, voltameters, &c.; and it is satisfactory to find that they correspond with the results obtained by the present instrument.

These laws being determined then, their operation in connexion with the transmission of electricity through atmospheric wires is, when sources of error are carefully excluded, very constant and definite; but in the action of subaqueous conductors, a new class of phænomena present themselves, in addition to and altogether different from those already referred to. A wire coated with gutta percha and plunged in water, represents a Leyden jar, of great length and small diameter. The wire is the inner coating, the gutta percha or other insulating substance is the dielectric, and represents the glass; and the water is the outer coating. If a coil of wire, insulated in this way, be immersed in a tank of water, with its two ends out of the water, we shall have a Leyden jar whose coated surface will depend upon the length and diameter of the included wire; and if a charge of electricity be communicated to this wire, either from an electrical machine or a voltaic battery, that charge will be retained for a certain time, and the wire may be subsequently discharged, producing effects commensurate with the conditions of the arrangement. I think about ten or eleven years since, I was applied to by an agent of the Gutta Percha Company, to explain the reason why a portion of the charge of a voltaic battery was retained by an insulated wire under the conditions which I have just described; and I at once referred it to the action of the Leyden jar. About two years subsequently it was submitted to Dr. Faraday, who gave the same explanation. From that moment I foresaw the difficulties which would present themselves when very long submarine lines should be used; and these have been constantly experienced, more or less, in all of

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them. In some cases they have been partially counteracted; but in others they have been so great as to render useless some most valuable instruments commonly used with the atmospheric lines.

The action may be thus described. Suppose an insulated wire extended for a great length under the sea, and having its two ends brought on shore and insulated. It is desired to work through this wire in the ordinary way, that is, with the earth for return circuit. The arrangement would be the following:—

A voltaic battery will have one of its ends connected with the earth, and the other with a key capable of making contact, when desired, with one end of the insulated wire. At the other end of this insulated wire, a telegraphic instrument will be so arranged as to receive the current from it, and transmit it to the ground; so that, according to some theories, the current originated in the galvanic battery, starts from one end, passes into the insulated wire, and tries to get back again to the other end of the galvanic battery; or what is the same thing, though not quite in accordance with the theory, tries to get at the earth or sea as soon as possible. If the insulation be perfect, the current is constrained to pass to the other end of the wire, and through the telegraphic instrument, before it can get to the earth; but if there be any fissures in the insulating coating, by which the electricity can find its way to the water, it will rather escape at once through them than force its way onwards through the resistance offered by the remaining length of wire, especially if that length be very great. Hence, if by accident or carelessness the gutta-percha coating of a submerged telegraph cable be defective, it is easy to understand, from what I have explained before, that although the conducting wire may be perfect, yet electricity sent in at one end may never reach the other, especially if the wire be disproportionately small in relation to its length, and consequently offer great resistance. But this is not the only difficulty or peculiarity incident to this arrangement.

The tendency which the electrical current, pervading the wire, has to escape into the sea throughout its whole length, sets up an inductive action between the conductor and the sea by which it is surrounded; and the conditions and actions of the Leyden jar are thus immediately established; and whenever a current passes through the conductor, that current necessarily charges the internal surface of the gutta percha with an electrical state bearing its own character, viz. positive or negative, according to the direction of the current. The amount of this charge will depend greatly upon the thickness of the gutta-percha coating, and the intensity of the current required to overcome the resistance of the wire.

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The charge thus communicated to the surface of the gutta percha endeavours to return into the wire whenever the latter regains its neutral condition; and if immediately after the transmission of a current through the conductor, its ends be connected with the earth, this charge will be found to flow out at each end, starting from a point near the centre, until the whole is discharged. If telegraphic instruments be so connected with the ends of the conductor as to form the channels by which these discharges flow back again into the earth, after each suspension of the battery current, they will, if they are constructed so as to be influenced by statical electricity, be acted upon by these discharges, just as if they had been transmitting signals from a battery. It follows, therefore, that before fresh signals can be transmitted with certainty and accuracy, the wire must be permitted to clear itself entirely of this residuary charge; otherwise the succeeding battery currents will be embarrassed and confused. In submarine cables of moderate lengths this difficulty has been partially met by modifying the character of the telegraphic instruments, but not without a considerable sacrifice of rapidity in working, as compared with the rate of transmission through atmospheric lines. With very long lines, however, even under the best circumstances, this clearing-time, or, as it is improperly called, retardation, is very considerable, amounting often to twice and thrice that required for the actual transmission of the primary current.

By a careful attention, however, to the due adjustment of the length and thickness of the wires to the battery current, much of this inductive action may be overcome, and the tendency to take up charge be diminished. When the resistance of a wire is very great, it requires high intensity in the battery current to overcome it; and as the tendency of the wire to charge the gutta percha increases in a much higher proportion than the statical intensity of the current, it follows that the lower this intensity can be kept (and this can only be done by diminishing the resistance), the less will be the embarrassment from residual charge. The only way in which this resistance can be diminished, is by increasing the bulk and conducting capability of the wire. It may be argued, however, that this increase of bulk increases the inner surface of the gutta percha exposed to the charging influence. Granted; but it must be remembered that doubling the diameter only doubles this charging surface, whereas the transverse sectional area, and consequent conducting capability, are quadrupled. The requisite intensity, therefore, and charging power of the current will be only one-fourth; and if the diameter of the wire were four times as great, the charging surface would also be four times as great, but the charging power of the current

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would be only one-sixteenth; and this I believe is very much within the mark, for reasons which I have before stated.

In applying these principles as a test in the examination of the Atlantic Cable, its construction appears objectionable in many respects.

I do not here intend to discuss the merits of its mechanical arrangement, such as the propriety or impropriety of coating it externally with wire, or its suitability or otherwise for the purpose of a deep-sea cable, though I consider that it is far from being the best form that might have been adopted for the purpose, since no provision is made to enable each element of which the cable is composed to take its own due proportion of the strain to which the whole might be subjected. I wish to confine myself more particularly to a consideration of its electrical qualities, which, from the very first, I have unhesitatingly disapproved of, not only publicly but privately in frequent friendly discussions with Mr. Whitehouse, the electrician of the Company, with whom, though I have the highest respect for his talents, I happen to differ very widely upon some important fundamental points.

I shall treat the subject in a dispassionate, scientific spirit, and deduce my reasonings from established electrical laws, and not from speculative theory. The first feature, then, which strikes the electrician, is the smallness of the conductor. It is a well-established electrical law, that the resistance which a wire offers to the passage of the electric current is directly as its length, and inversely as its transverse sectional area, or, in other words, inversely as its mass. A wire of double the mass, and the same length as another, will conduct twice as well, it being equivalent to two wires laid side by side. A wire of the same thickness, but twice the length of another, will conduct only half as well; therefore if doubling the length reduce the conducting power one-half, it is only requisite to double the mass of the wire which is twice as long, to bring it up to the original standard of conducting power. By parity of reasoning, if a wire of given mass, and 200 miles in length, and possessing a certain amount of conducting power, have its length increased tenfold, its mass must be increased tenfold also, in order to maintain the same conducting power relatively to the electro-motive force of the battery working through it. If, with the increased length, its mass be only increased fivefold, then it will possess double the resistance, and will require a battery-current of twice the intensity to overcome it; or if the length be increased ten times without increasing the mass at all, then the battery power must be increased ten times, or telegraphic instruments of ten times the amount of susceptibility must be used. The wire of the Atlantic Cable is composed of seven small wires, of No. 22 gauge,

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twisted together, forming a strand or cord about equivalent in mass and conducting power to a copper wire of No. 15 or 16 wire-gauge, the size of tolerably stout bell wire. This is not so thick as would be used on overland lines if copper were employed instead of iron. Now even with the best insulation, and the most favourable conditions of which these overland wires are susceptible, it is found that currents of high intensity are constantly required in long lines; yet it appears extraordinary that this fact should have been overlooked in the determination of the size of the Atlantic wire, placed as it is under conditions which tend to divert the electrical effects of the currents through every inch of its length.

In addition to the great intensity of current requisite to overcome this unprecedented amount of resistance, a still greater degree of electrical force was necessary to compensate for the absorbing or inductive influence of the insulating coating in close contact with its surface.

To meet this difficulty Mr. Whitehouse contrived his induction coils, with a view of obtaining from their secondary currents electricity at a degree of tension which should be adequate for all the requirements of the cable. Unfortunately these coils, of which I shall have to speak hereafter, were constructed upon principles purely hypothetical, and were not the result of the experience derived from practical investigation, or carefully and gradually developed plans.

The disadvantages arising from this attenuated form of conductor are as follow:—

1st. Its great resistance requires the employment of electricity of very high tension, involving either the use of batteries in very extensive series, or of electro-magnetic or magneto-electric machines of great power.

2nd. Supposing the first difficulty to be overcome, which is quite practicable, another disadvantage presents itself, viz. the necessity of adapting the recording instruments to the character of the current. Electrical effects are of two kinds, designated by the terms static and dynamic, embracing the two extremes of intensity and quantity. Magnetic effects depend upon the dynamic or quantity character of the current, and when they are required to be produced from statical electricity, or intensity currents of low dynamic character, they can only be obtained by multiplying a great number of statical effects, so as to get the united actions of the minute quantity due to each.

Since with a long attenuated conductor, currents of high tension alone are available, the recording instruments must be such as to be influenced by statical or high-tension electricity; and the result is, that in proportion as they are so, they are not only

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acted upon by the primary electric current intended to produce the signal, but by the static charge which has been taken up by the gutta-percha coating from the wire whilst the electricity was passing through it, and which returns into the wire and escapes through it, and through the instruments, after the primary current has passed, and which continues to influence them until the whole of that residual quantity has been discharged.

3rd. The static charge which is taken up from the wire by the gutta-percha coating increases even in a higher proportion than the intensity of the current itself, and therefore with a long attenuated wire these static effects increase, not only with the length of the wire, but with the intensity of the current working through it, and as gutta percha is analogous to crown glass and some other insulating substances in its power of taking up electricity quickly, and parting with it again slowly, the delay occasioned by waiting for the wire to clear itself of this static charge and regain a neutral condition suited for the transmission of a new current, is so considerable as to interfere most seriously with the rapid transmission of signals. The recording instrument, after being affected by the primary current, remains still acted upon by the residual charge, though in a gradually decreasing degree for a second or more, and consequently no new signal can be transmitted whilst these effects are taking place, whereas with an overland line the transmission of the electrical impulse is so instantaneous and abrupt, that as many as twenty or more distinct impulses can be recognized and recorded in a single second.

4th. This disadvantage leads to another, viz. the necessity of working very slowly, and employing recording or indicating telegraphs, the varied signals of which combine to form letters; and as these letters are often composed individually of four or five separate signals, each occupying a second or two for its distinct and perfect transmission, and as words contain on the average five or six letters, it follows that each word will require from fifty to sixty seconds for its transmission. This was actually the rate at which, under the most favourable circumstances, the Atlantic Cable worked in Keyham Dockyard, although from various newspaper reports the public were led to believe that as many as four words per minute had been transmitted through it. This might probably have been the case if words only had been selected composed of one or two signals, but in ordinary messages one word per minute was the average rate of transmission.

The loss of force in the current of electricity by the resistance of the long wire, may be comprehended when I state that a flow of electricity from a pair of huge induction coils capable of producing the brilliant combustion of thick pieces of copper wire

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when passing between their terminals, and sufficient, I should consider, to destroy life in an instant, was so reduced when passing through 2500 miles of the Atlantic Cable, that I could just perceive a slight throb in my tongue whilst allowing the shock to pass through it.

An attempt was made to obviate the embarrassment arising from the action of the residual charge, by reversing the currents, and upon the following principle: viz., that as the passage of a positive current through the wire charged the internal surface of the gutta percha positively, giving rise to a residual discharge of positive electricity from the wire after the passage of the current itself, so it was thought that the transmission of a negative current after the positive, instead of another current of the same character, might have the effect of assisting, as it were, in more rapidly exhausting the residual positive charge, and disincumbering the succeeding negative current of disturbing influences. This plan partially succeeded, but only partially, since the time saved in the transmission of consecutive signals was but little. It was found, practically, that when these reversals were repeated quicker than at certain intervals, no signals were indicated. I had a remarkable opportunity of testing this peculiar effect whilst examining some of these phænomena with Mr. Whitehouse. When the conductor of the Atlantic Cable was separated in the middle, and the two ends laid upon the tongue, so that it should form part of the circuit, the effect of the reversals could be easily and curiously distinguished. Each wire produced a sensation, the negative one being the stronger. As the reversals were made, so the characteristic pungent sensation alternately shifted from one wire to the other on the tongue, and as they followed each other more rapidly, so the sensations became less and less distinct. From practice however, added perhaps to a greatly increased nervous susceptibility, arising from my want of sight, I was able to appreciate these alternations long after they would have ceased to be indicated by the recording instruments.

The reason of this partial success appears evident enough, on considering the nature and action of the residual charge. It must be remembered that a wire coated with gutta percha and immersed in water, has a double office to perform, viz. not only to conduct the current to the other end, but to distribute portions of that current throughout its course to the surface of the gutta percha, for the purpose of charging it after the manner of the Leyden jar; and since the terms positive and negative, when applied to the current, are merely conventional, and only indicate its direction, it is necessary to consider the phænomena in their relation to these directions. A voltaic battery, then, or any other arrangement which sets electricity in motion, is possessed, accord-

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ing to the single-fluid hypothesis, of the power of giving out electricity at one end, and taking it in at the other, and if a very long wire have its two ends brought in contact simultaneously with the ends of a voltaic battery or other electro-motor, the first action is an exhausting effort at one end, and a flow of electricity into the opposite end, thus disturbing for a brief instant the normal distribution of the electricity naturally belonging to the wire. However rapidly these effects may pervade the whole length of the wire, there is a time when the ends and the centre will present three different degrees of electrical condition, the centre being neutral, and the ends respectively plus and minus. Now, suppose contact with the galvanic battery to be made with one end of the wire, only its other end being in connection with the earth at a very remote distance, the electrical conditions of the wire will be different.

If contact be made with the plus end of the battery, a flow of electricity takes place into the wire, producing a wave which gradually flows to the other end, charging the gutta percha in its passage in proportion to the intensity of the current required to overcome the resistance of the wire. That this occupies time, is proved by Mr. Whitehouse's ingenious chronometric test, which registers the time at which the current appears in different portions of the length of the wire. The electrical condition of the wire will in this case be different: the remote end will be neutral until the current reaches it, but the other end will partake of the plus condition of the end of the battery; and after the current has pervaded the wire, the whole will appear positively charged. If contact with the battery be now broken, and that end of the wire be also made to communicate with the earth, the wire, as far as itself is concerned, instantly becomes neutral, but the charge from the gutta percha now returns to the wire, and flows out at both ends from the centre in opposite directions, giving rise to two currents at the ends of the wire, one at the remote end in the same direction as the first current of the battery, and another out at the near end in opposition to the original current of the battery.

If this experiment be reversed, and the negative or minus end of the battery be brought in contact with the free end of the wire, the other being to earth, a partial exhaustion, as it were, of the electricity natural to the wire takes place, which effect gradually extends to the other end, so that the current is produced not by propulsion from the battery, but by exhaustion towards it. As soon as the effect pervades the whole system, therefore, it appears minus or negatively charged, and on breaking contact with the battery and communicating the second end of the wire to earth as before, two opposite currents are again produced, entering the

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two ends and flowing towards the centre to supply the gutta percha with the electricity which the wire and battery have extracted from it.

Now it is quite evident that a second contact of the battery with the wire whilst charged in either of the preceding conditions, must be attended with results very different from those of a contact with a neutral wire. In the first case, viz. that in which the two currents are flowing out of the wire, the second contact of the plus or positive end of the battery will have to react against the positive current flowing out in that direction, and cause it it to return and flow out at the other end, and, following close after it, will, when it reaches the remote end, merely produce an effect equivalent to a continuous current without a break or an interval between. If, on the other hand, the negative end of the battery be brought in contact with the positively charged wire, as in the case of a reversal, the effect will be that its exhausting influence will first facilitate the issue of the positive current from the end of the wire with which it is brought in contact, and it will then begin to extend its influence to the remote end of the wire, following, as it were, upon the heels of the positive current going out at that end, and calling back portions which might otherwise have continued in that direction.

With a negative charge in the wire, of course the converse of these actions takes place. So much time is occupied in the transmission of a wave through the Atlantic Cable, that it is easy to send a positive current in at one end, and, before it shall have reached the other, to arrest it and cause it either to subside or return, by reversing the connection, and substituting the negative or exhausting end. I am using familiar terms, because these remarks may meet the eyes of the unscientific as well as the scientific, and I wish to be comprehended by both. It thus appears that, whether consecutive currents of the same character are sent forward, or reversals of the currents are employed, more time is necessarily consumed than is commercially desirable, and the value of the cable is hence considerably depreciated.

I have alluded to the property of gutta percha to retain an electric charge, known as its specific inductive capacity. This property adds to the embarrassment; for although gutta percha takes up an electric charge very readily, yet that charge appears to penetrate into its surface, and entangle itself in its pores, to such an extent that it separates from it again with reluctance. I have before drawn attention to an analogous property in crown glass (Phil. Mag. April, 1858), which retains as much as 25 per cent. of the original charge, and parts with this residue with great difficulty and in small portions at a time, so that, after a coated plate of crown glass has been charged and discharged, it

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will yield as many as 20 or 30 minute residual discharges, extending over an interval of half an hour.

Having now pointed out what I consider to be the chief scientific defects in the Atlantic Cable, I might proceed to describe the means which I have lately introduced for removing them, and obviating the difficulties which at present lie in the way of the successful construction and working of very long submarine telegraph cables. But I intend this to form the subject of a distinct communication; I shall therefore now proceed to examine the arrangement and peculiarities of the instruments intended to be employed for working through it.

When intense currents are wanted to overcome resistance, it is necessary to use batteries consisting of a great number of elements; but as a highly-resisting conductor can transmit only a small quantity of electricity, these elements may be extremely small, and I believe that the batteries usually employed are very much larger than necessary. There are other modes of exciting electricity of high tension, where the quantity effects are not required to be great, such as the secondary current of an induction coil, or the current produced from a magneto-electric machine. I see no reason, however, why, small and inadequate as the Atlantic conductor is, it might not have been worked with an intensity battery of a large number of small plates; but the electrician of the Company, Mr. Whitehouse, preferred working with electro-magnetic coils, and accordingly contrived an induction coil for the purpose, having the primary wire outside and the secondary wire within, immediately surrounding the core.

From a careful consideration of this instrument and its effects, it appears to me open to many objections, both as regards its electrical arrangements and mechanical construction; and the comparatively small amount of effect produced by it in relation to its magnitude, and the enormous power and gigantic character of the batteries required to excite it, seem to justify these conclusions, and to indicate that there are some serious radical defects in the internal arrangement. Judging from the power developed from my own form of the induction coil, I was prepared to expect effects some fifty times greater. When, however, Mr. Whitehouse explained to me that none of the coils had been properly tested, and that some of the largest had even been made and put on board ship without any trial whatever, from want of sufficient time and opportunity, it was easy to understand how apparatus requiring such an intimate and profound acquaintance with the laws of electricity on the part of the inventor, and so much mechanical skill and judgment added to the greatest familiarity with electrical appliances and arrangements on the part of the

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workmen employed, might fall very far short of what was originally expected. Indeed the induction coil is an instrument the success of which depends so much upon the experience derived in the course of repeated manufacture, that the greatest wonder is that Mr. Whitehouse's coils have succeeded at all under such disadvantageous circumstances. They possess the elements of enormous power, if judiciously arranged and constructed; but since they give such unmistakeable evidence of defective construction, it would be hardly fair to attribute their present failure to a faulty arrangement. I do not refrain, however, from stating my general objection to every portion of the plan upon which they are made. One peculiarity in particular, which renders them totally unfit for the purpose for which they are intended, is that the secondary current is deficient in intensity, and that its quantitative effects are not only far too great for the required purpose, but the current itself, instead of being abrupt and instantaneous, possesses an amount of duration quite incompatible with the rapid reiteration of signals. In some experiments which I witnessed, the secondary current, in passing between large copper terminals, flowed for more than a second, producing a vivid combustion, which permitted the terminals to be gradually separated from each other to a distance of three-quarters of an inch. A current capable of flowing for so long a time through such a resisting medium as the atmosphere, would flow still longer through a conductor, and would thus add greatly to the difficulties already presented, as I have shown by the construction of the Atlantic Cable.

In order to provide a current suitable for the capacity of the enormous primary wires of these induction coils, gigantic batteries were constructed, consisting of 400 plates of silver 9 inches square, and the same number of similar plates of zinc, which were fitted into 20 gutta-percha troughs, each containing 20 alternations of zinc and silver. The 20 silver and 20 zinc plates in each trough were arranged as single pairs, all the silver being united at the top, and all the zinc at the bottom. The whole battery thus consisted of 20 pairs of plates, each containing $22\frac{1}{2}$ square feet of silver, calculating both sides in action. These stupendous batteries were mounted in ponderous iron gimbels for the sake of stability on board ship; the cost of the silver was about £2000, and that of the whole batteries, independently of coils or other apparatus, about £3000. Subsequently, however, from some experiments with plates of gas carbon, it was discovered that these were more energetic in their action than silver plates, and accordingly the electrician of the Company deemed it advisable at once to discard all the latter, and introduce plates of gas carbon in their place.

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Bearing in mind the tiny character of the Atlantic wire, one is irresistibly led to inquire what end such a battery as this was destined to accomplish, and whether the same end might not have been attained by much smaller means.

Its object, then, is not to generate a current of electricity to be passed through the cable, but through the primary wires of the induction coil, in order to excite magnetism in its iron core; and it is the magnetism thus excited which has to react upon the secondary coil, and generate the current of electricity which is to be employed for working through the cable. The electrician will not fail here to predicate many chances of loss of power, if the conditions requisite for developing the greatest amount of magnetic power in the iron core, as well as for turning to the best account the magnetism thus obtained in the production of a secondary current, be not observed. The effects at present produced by these induction coils, as I have before remarked, indicate serious losses somewhere; but whether they arise from a faulty principle or defective workmanship, is a problem yet to be solved. I cannot conclude this paper without offering an opinion or two on the present cause of failure of the Atlantic Cable, and the ultimate prospect of success. Had the cable been tested in water, after completion, which might have been readily done at Keyham Dockyard, defects might have been easily discovered and repaired. The omission of this test leaves much room for speculation as to the cause or seat of the injuries or defects. I have no faith in the modes which have been adopted to discover their situation, so far as I have become acquainted with them, though I believe that the proximate determination of these particulars is still attainable. A consideration of the mechanical construction of the cable shows that it is very liable to injury in the process of laying. I have seen some specimens recovered after immersion, which were kinked in such a manner as to strain and injure very materially the gutta-percha coating of the conductor, which having nothing but its own tenacity to depend upon, would be subject to enormous tension by the lengthening of the external iron covering. With electricity of such high tension as that required to work through the wire, the smallest fissure or defect in the insulating coating would form a leak of a much more formidable character than if it existed in a wire of moderate length; and the fact of working to earth increases the tendency to lateral discharge. If, however, the faults be not discovered and remedied, the cable, although useless for the purpose for which it was originally intended, may still render valuable assistance to the success of future lines by being employed as a wire for the return current, instead of employing the ordinary mode of working to earth—a practice which appears to me, in relation

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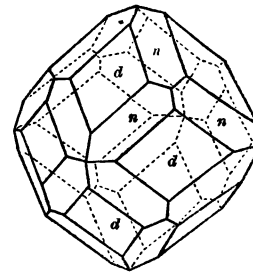
to submarine cables, to be highly objectionable. The employment of a return wire, especially of large conducting capacity, would prevent much of the inductive action which now takes place between the inner conductor (the wire) and the outer conductor (the sea). I believe also that a current of moderate quantity and high tension, such as is developed in my own form of the induction coil (Phil. Mag. Dec. 1856), would be far better calculated to overcome the difficulties met with in the Atlantic or other submarine cables, than the contrivances which have been hitherto adopted.

V. *A List of the Pseudomorphic Minerals found in Scotland.* By Dr. Heddle*.

SHETLAND ISLANDS. In Mainland, on the west side of Hillswickness, nearly opposite the Drongs,—

Chlorite after garnet, form *d n*†, fig. 1.

Fig. 1.



Although the garnets are here frequently an inch in diameter, yet the crystals which have been metamorphosed are those of about the size of a pea, and the altered crystals occur only on the exterior of the rock,—the modifying agent, here evidently external, not having penetrated above three inches in depth: taking this into consideration, along with the situation of the crystals, and seeing that the chief change from an almadine garnet to chlorite consists in a diminution of silica, increase of magnesia, and addition of water, we may conclude that the change has been due to the action of the sea.

Limnite after pyrites, form P *e*, fig. 2.

In Unst; on the north side of Balta Sound, in the large chromite quarry,—

Serpentine after chromite, form *o*, fig. 3.

At the smaller quarry at Hagdale,—

Kammererite after talc, form *a o*, fig. 4.

* Communicated by the Author.

† The lettering is that adopted by Greg and Lettsom in their Manual of the Mineralogy of Great Britain and Ireland.

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Fig. 2.

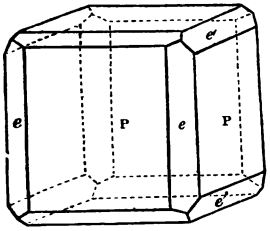


Fig. 3.

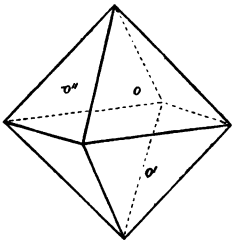
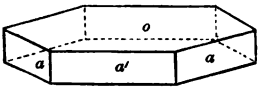


Fig. 4.



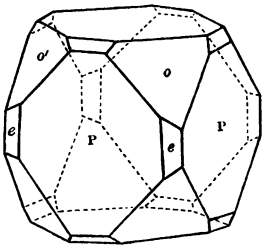
Orkney Islands. In Pomona, on the shore west of the point of Ness near Stromness,—

Limnite after cockscomb marcasite.

In Hoy, in the cliffs facing Braebrough, Hoy head,—

Hæmatite after pyrites, form P *o e*, fig. 5.

Fig. 5.



Aberdeenshire. At Glen Gairn, along with idocrase and Wollastonite (?)—

Essonite after epidote seemingly, the form being indistinct.

Perthshire. At East Tulloch and elsewhere,—

Limnite after pyrites, forms P and P *e*, figs. 6 and 7.

Fig. 6.

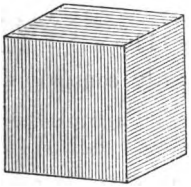
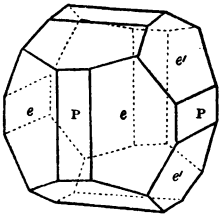
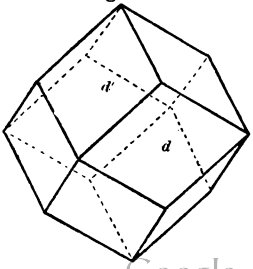


Fig. 7.



The crystals with the form P exhibit beautifully the striæ dependent on an oscillation between the faces of the cube and pentagonal dodecahedron.

Fig. 23.



On the Knock behind Ballantuim Strathardle,—

Chlorite after garnet, form *d*, fig. 23.

Fifeshire. Near St. Andrews,—

Marcasite after mineral charcoal.

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This interesting pseudomorph was found at the Spindle rock, where an outburst of trap tufa has passed through the lower coal formation, carrying up with it and altering portions of its various members: a rhomb of coal was found completely converted into marcasite, which beautifully imitated the filaments of mineral charcoal scattered through the mass.

At Glenfarg, hollow pseudomorphic cavities of what have been rhombs (*f*) of calcite occur in fargite, but in no instance filled with any substance, the pseudomorphic change being here incomplete.

Isle of May, in the cliffs on the west side, with datholite,—

Prehnite after scolezite, form indistinct.

Dumbartonshire. At the Long Craig,—

Weissigite (albite) after stilbite, forms *r a b*; *r a b* P; *r a b* P M: figs. 8, 9, 10.

Fig. 8.

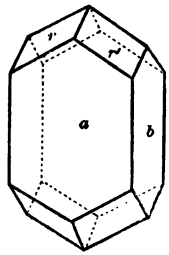


Fig. 9.

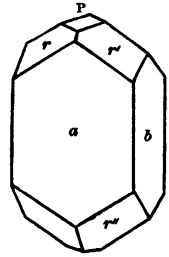
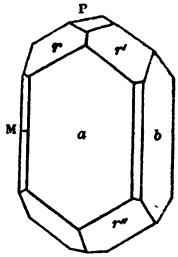


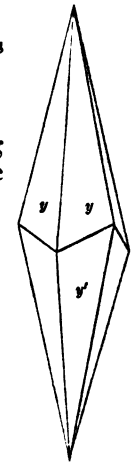
Fig. 10.



Analcime after stilbite, form *r a b*.

Analcime after calcite, form *y*, fig. 11.

Fig. 11.

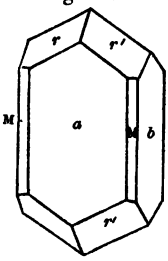


The interior of these crystals frequently consists of unaltered calcite.

Quartz after stilbite, form *r a b*:

this is, however, more of the nature of a coating than a replacement of substance, as the crystals are for the most part hollow.

Fig. 12.



A white substance (hardness about 6·, pulverulent, insoluble in acids, containing silica, alumina, lime, soda, and water) after stilbite in the forms *r a b*; *r a b* M (fig. 12); *r a b* P M.

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Kilpatrick Hills,—

Prehnite after analcime, forms *n*; *n* P: figs. 13 and 14.

Fig. 13.

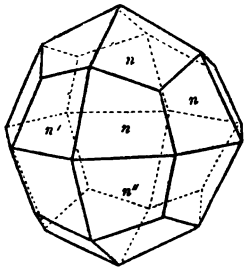
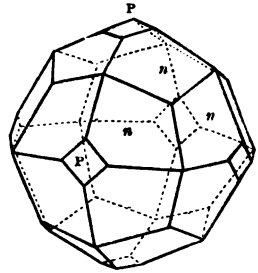
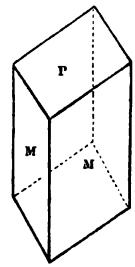


Fig. 14.



These pseudomorphs are white, but their interiors are sometimes hollow, the inner surfaces being ordinary botryoidal Prehnite of a fine yellow colour.

Fig. 15.



Analcime after Laumonite, form P M, fig. 15; also in a highly modified form, apparently *e x m b u r*, but indistinct from the small size of the crystals.

These pseudomorphs are usually considered to be analcime (variety sarcolite), and are sometimes sold under the name of "Cluthalite;" I am of opinion that they will prove to be Weissigite, a zeolitic or fused albite. No mineral at all answering either in characters or composition to the cluthalite of Thomson has of late years been found near the Clyde, though both the above and the Weissigite pseudomorphs have passed as such.

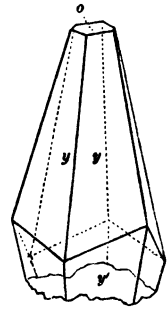
Prehnite after Laumonite, form P M.

Weissigite after Prehnite (?)

Steatite after natrolite.

Green earth after calcite, form *y o*, fig. 16.

Fig. 16.



Haddingtonshire, near Tantallan Castle,—

Celestine in radiating tufts after some mineral unknown; ? natrolite.

Edinburghshire. In Ratho quarry,—

Pectolite after analcime, forms *n*; *n* P.

Barytes after analcime, form *n* P.

Steatite after barytes in tabular crystals.

Steatite after analcime, form *n*.

Steatite after radiated pectolite.

Ayrshire. At Landelfoot,—

Pectolite after scapolite.

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As the crystals are rough (they occur in quantity too small for accurate investigation), it is not improbable that the substance as well as the form is that of scapolite; if so, it is the only locality in the British Islands at which that mineral has as yet been observed.

Lanarkshire. At the Lead-hills,—

Galena after pyromorphite, form *a o*, fig. 17.
Quartz after Anglesite, in long bladed crystals.
Quartz after barytes, form indistinct.
Quartz after psilomelane, botryoidal.
Quartz after galena, form P, fig. 18.
Cerussite after galena, form P.
Chrysocolla after galena, form P.
Chrysocolla after cerussite, form M *a* P *i b p*, fig. 19.

Fig. 17.

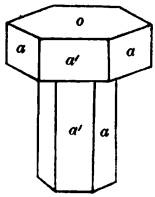


Fig. 18.

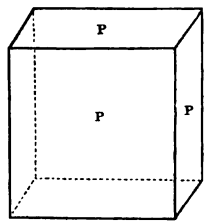
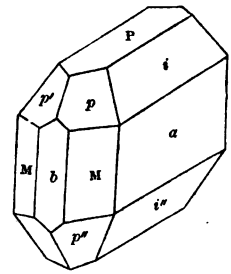


Fig. 19.



Minium (ferruginous) after galena, form P.
Calcite after galena, form P *o*, fig. 20.
Wad after calcite, form *d u*, fig. 21.

Fig. 20.

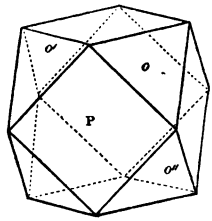
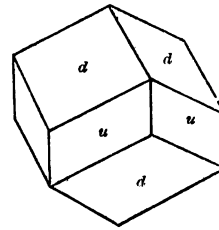


Fig. 21.



This unique specimen, which is now in the cabinet of Mr. Dudgeon of Cargen, is most beautifully studded with minute crystals of Arragonite, all of which are disposed over the surface of the pseudomorph, so that their long axes are parallel to the plane *u*.

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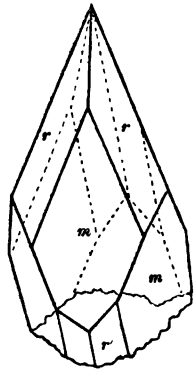
Vanadinite after galena, form P.
Calamine after vanadinite, form *a o*, fig. 4.
Hematite after calcite, form *m r*, fig. 22.

Fig. 22.



Argyllshire. On the east shore of Kerrara Sound, in argillaceous schist,—

Hæmatite after pyrites, form P.
The centres of these pseudomorphs, which occur in large quantity, are sometimes hollow.

On the island in Kerrara Sound, and on Kerrara,—

Limnite after pyrites, forms P, P*e*.

About three miles north-west of East Tarbet,—

Magnetite after pyrites, forms P, P*e*.

Patches of unaltered pyrites occur dispersed throughout the mass of some of these crystals, which are apparently associated with small quantities of pennite and emerald nickel.

VI. *An Inquiry into the Origin of Whirlwinds.*

By Thomas Belt, *Mount Egerton, Victoria**.

HAVING during the last four years studied the phænomena of those small eddies of wind common in many parts of Australia during the summer months, and having deduced principles which are, I believe, applicable to the solution of all, or nearly all, circular movements of the atmosphere, I am induced to lay the results of my observations before your Society, hoping that they may tend to clear up some of the doubts entertained concerning the origin of circular winds.

Lying, as Melbourne does, within the limits of one of the great hurricane tracks, the subject is one of great importance; and even if the opinions I am about to lay before you prove erroneous, still good must ensue from the attention of your Society being directed to the subject.

The small eddies of air of common occurrence in this colony, are examples of the simplest form of a whirlwind. Taking them, therefore, as a starting-point, I shall propose a theory to account for their violence and circular action, and then seek to apply the same to the elucidation of the grander convulsions of the atmosphere.

Much of my reasoning must of necessity be from analogy;

* Read before the Philosophical Institute of Victoria, December 1857; communicated by the Astronomer Royal.

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but I believe I shall not incur the charge of going beyond the limits of legitimate speculation.

Every resident in Australia must have observed, during the hot season of the year, eddies of air carrying up dust, leaves, and other light substances to a great height, appearing at a distance like moving columns of dust. Though only a few yards in diameter, they are of great violence, often unroofing or overturning the slight tents of the gold-seekers. The dust and leaves they carry up render their upward spiral motion very conspicuous. The columns sometimes remain stationary, but generally they have a regular horizontal movement. Clouds of dust envelope their base, out of which they rise to a considerable height, often bent out of their perpendicular by upper aërial currents. They are especially frequent on the level plains, where, from the absence of trees, the rays of the sun exert great power.

Small whirlwinds or eddies of air are not peculiar to Australia. Humboldt speaks of some observed by him on the llanos of South America, and ascribes them to the meeting of opposing gusts of wind. The vertical columns of sand seen by Clarke on the steppes of Russia, and by Bruce over the deserts of Africa, are similar phænomena. They sometimes, but rarely, occur in England, carrying up loose hay and other light substances, and scattering them over the surrounding country. Franklin describes a whirlwind that he witnessed in Maryland, which began by taking up the dust that lay on the road in the form of an inverted sugar-loaf, and soon after grew to the height of forty or fifty feet, being twenty or thirty in diameter. It advanced in a direction contrary to the wind; and although the rotatory motion of the column was surprisingly rapid, its onward progress was so slow as to allow a man to keep pace with it on foot. Franklin followed it on horseback, and saw it enter a wood, where it twisted and turned round large trees; boughs and leaves were carried up so high, that from their height they were reduced to the apparent size of flies. This last, though a much more violent whirlwind than those experienced in Australia, is strictly analogous to them.

If those eddies of air are attentively observed, it will be perceived that currents of air are moving along from all sides towards the lower apex of the column. The temperature of the air next the surface is sensibly diminished by their action. Often when travelling over the parched plains, I have seen the air quivering over the hot ground as from a furnace; suddenly (within a few paces perhaps) a miniature storm has arisen; and when after a few minutes' violence it has as suddenly ceased, the quivering of the air has been no longer perceptible, and the atmosphere has felt less oppressive: again and again the same

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process has been repeated, until the conclusion became inevitable, that those whirlwinds were the channels that carried off the heated air from the surface to the higher regions.

It is generally supposed that, as the strata of air next the surface become heated, the rarefied particles rise and are mixed with the higher and cooler layers; but I expect to be able to show that this equalizing action does not always take place in such a regular and placid manner, but is accompanied by, and the cause of, those commotions of the atmosphere known as hurricanes, typhoons, and whirlwinds.

In calm or nearly calm weather during the summer months in this colony, the strata of air next the ground become heated; and unless there is sufficient wind to carry them off and mix them with cooler portions of the atmosphere, they remain next the surface in a state of unstable equilibrium, and heat goes on accumulating until the elastic force of the heated strata becomes so great, that at special points, where some peculiarity of the ground has favoured a comparatively greater accumulation of heat, they are enabled to pierce through the overlying masses of air and force their way upwards. An opening once made, the whole of the heated strata will move towards it and be carried off, the heavier layers sinking down and pressing them out.

The eddies of air I have described, are the points where the heated air from next the surface is escaping through the denser superincumbent atmosphere; and as it has not only to force upwards but to contend against the pressure on the sides of the ascending column, it will readily be perceived how this double action gives to it its rotatory or spiral motion. The behaviour of an eddy of air is similar to what occurs when an opening is formed through the bottom of a shallow cistern of water.

As heat is an active agent in eliciting electrical action, it may be that the lower strata are prevented from mixing with the upper by their peculiar electrical conditions.

The hot winds of Australia prove that air does not always rise as it is heated, for in them we find a warm current of air actually displacing the cooler atmosphere.

Analogy with hurricane, &c.—If the violence of the whirlwind is caused by the great pressure of the upper masses of air forcing out the lower strata, its dimensions and force will be in proportion to the extent of the rarefied layers,—so that whenever large tracts of air next the surface are liable to be gradually heated, we may expect whirlwinds to occur at intervals similar to the Australian eddies (which may be called the initial phase of a whirlstorm), but of greater extent and violence in proportion to the larger tracts of air drained off.

Perhaps the next in violence to the eddies of air is the whirl-

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wind of the sandy deserts of Africa, the dreaded Simoom. This is the outlet of the heated air extending over the surface of the dry desert. It is the presence of this heated stratum that causes the singular delusion of the mirage. In it we may perceive the couching simoom luring on the weary traveller with false hopes of arriving at refreshing sheets of water, then rising in its fury and overwhelming man and beast in a mound of sand. Bruce, speaking of the whirlwinds of the African desert, says, "We saw towards the north a number of prodigious pillars of sand at various distances, sometimes moving with great velocity, sometimes stalking on with majestic slowness." Another traveller had an opportunity of seeing one of these pillars crossing the river Gambia from the Great Desert. It passed within eighteen or twenty fathoms from the stern of their vessel, and seemed to be about 250 feet in height. *Its heat was sensibly felt;* and it left a strong smell like saltpetre, which remained a long time.

It is, however, over the expanse of the wide ocean that we find the greatest development of the whirlstorm, namely, the typhoon and the hurricane.

Since the circular action of these storms was demonstrated by Redfield, the interests of navigation, as well as the requirements of science, have caused great attention to be paid to the subject. The tracks of many of the meteors have been defined, and minute directions laid down for the guidance of navigators, so that they may avoid the centre or vortex of the storm.

Yet though many opinions have been put forward to account for their origin, so unsatisfactory are they considered for the solution of all the phænomena accompanying those meteors, that many writers on the subject concur with Colonel Reid, that "on the cause of storms, in the present state of our knowledge, it is best to be silent."

When, however, we apply the theory I have exemplified in the Australian eddies, the solution of the characteristics of the whirlstorm is complete and simple. As, on the land, whirlwinds and eddies are most numerous where the sun exerts most power and the atmosphere is least agitated by winds, so over the ocean we find the regions of the cyclones existing under similar conditions. It is within a few degrees on either side of the equator that the cyclones originate and are most violent. The hurricane season in the northern hemisphere extends over the months of July, August, September, and October; and in the southern hemisphere, over December, January, February, and March; and we find the hurricane tracks to be over areas of the ocean shut off by the interference of land from the continual action of the trade-winds and their equalizing influence. The cyclone region of the

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Atlantic is within that great bight formed by the coasts of North and South America, having for its apex the Gulf of Mexico; and that of the Indian Ocean is bounded by an arc of land having at one end the continent of Australia, at the other the island of Madagascar and the southern termination of the continent of Africa.

In these and suchlike regions of the immense ocean the materials for the hurricane are piled up. Here, from day to day, the lower atmosphere is gradually heated by the direct rays of the sun during the day, by irradiation from the sea during the night. As in Australia the quivering of the air over the hot dried ground precedes the eddy, and in Africa the mirage foreshadows the simoom, so the close stifling atmosphere in the West Indies foretells the hurricane. The hurricane is the breaking up of a continuance of warm weather, which at the latter end has been exceedingly sultry. This fact of itself is sufficient proof that the air next the surface does not gradually rise as it is heated; if so, the temperature would be comparatively equable, and no such accumulation of heat could take place. Whilst on this part of the subject, I may mention that the temperature of the atmosphere has not been sufficiently considered in treating of hurricanes. In every account of a cyclone we find minute readings of the barometer, whilst the thermometer is almost neglected. Now, although the barometer is invaluble to the mariner for indicating the approach of a hurricane, yet the latter is the effect of elements whose quiescent existence is shown by the thermometer; and it is for the interest of science that greater attention should be paid to it in those regions where cyclones originate, to do away with our present defective information on the subject.

It is a well known feature of cyclones, that they rotate in opposite directions in the two hemispheres. In the northern this direction is E., N., W., S., whilst in the southern it is E., S., W., N., or contrary to the apparent course of the sun. It is this constancy in the method of their rotation that enables the skilful mariner to calculate his safest course when he encounters a whirlstorm. I have seen no attempt to account for this constant element in the action of cyclones, excepting on electrical grounds; but as we know that the Australian eddies rotate indifferently in either direction, we must find some solution applying only to the larger meteors.

At the commencement of a cyclone, when an opening is forced through the overlying atmosphere, the heated strata, extending over a large area, rush towards the focus from all sides, and those currents of air are turned out of their direct course by the action of the earth's rotation in the same manner as the

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trade-winds are affected. Thus, suppose the cyclone A originates a few degrees south of the equator, and B and C are currents of air moving towards it in opposite directions. The course of the current B is from south to north; but inasmuch as the parallels of the earth's surface, over which it moves, increase in velocity as it approaches the equator, and it does not acquire this accelerated motion, it lags behind, and assumes a direction west of the point towards which it is moving.

The current C, on the contrary, is coming from regions having a greater velocity than those at which it is constantly arriving, so that it acquires an impetus towards the east; and this impulse is sufficient to determine the direction of the rotation of the storm, which would otherwise be liable to take either course indifferently. In the northern hemisphere it is evident that the rotation of the earth has an opposite effect upon the meteor.

As the cyclones progress towards the poles, they rapidly increase in diameter and decrease in violence. Thus, when a hurricane is met with within the tropics, its diameter will not exceed 300 miles; when it has reached the 50th parallel, it will extend over 1500 miles. It is not only the ascending column of air that acquires the rotatory motion, but the whole of the air moving towards it must partake of it, and thus the dilatation of the whirl increases as long as the cyclone lasts. It is probable that, compared with the extent of the whirlstorm, the ascending column is of very small diameter.

Concerning the recurving of the cyclones somewhere about latitude 30° in both hemispheres, I will only remark that I do not consider it to be an inherent feature of a whirlstorm, but rather to be impressed upon it by the line of land forming the boundary of the cyclone region. And this opinion is borne out by the fact, that the cyclones on the eastern coast of Australia recurve towards the west, following the line of coast.

The electrical commotion and the heavy showers of rain and hail accompanying a cyclone may be briefly glanced at. During the heating of the lower strata of the atmosphere, and consequent evaporation over the surface of the ocean, the vapour so generated will be partly diffused throughout the heated strata, and will partly be formed into clouds on their higher limits. When the cyclone bursts forth and the air rushes upwards, the vapour at the outer edges of the vortex, where it comes into contact with the colder atmosphere, will be precipitated in rain; other portions will be carried so high into the upper regions of the atmosphere that they will not only be condensed, but congealed, and fall in showers of hail. The vapour carried from the surface of the ocean must be highly charged with electricity, which, as

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the vapour becomes condensed, will be discharged in lightning; so that the electrical commotion always observed during the action of a cyclone is the effect, and not the cause, of the atmospherical commotion.

It may be objected to the theory I have advanced, that severe rotatory gales are experienced in Great Britain in the depth of winter. These storms have, however, originated in warmer latitudes, and seem to follow down the course of the great Gulf-stream, the warmth of which is brought into greater contrast at that season with the surrounding regions. It must also be borne in mind, that although the air may be very slightly volatilized, so as to be utterly inadequate to originate a cyclone, yet it may be quite sufficient to sustain one in action.

VII.—*Biographical Notice of the late* Richard Taylor, F.L.S. &c.

IT is this month our painful duty to record the death of Mr. Richard Taylor, who for a period of thirty-eight years has assisted in conducting this Journal, having become joint editor with Dr. Tilloch, the founder of the 'Philosophical Magazine,' in the year 1822. On a future occasion we shall endeavour to do more ample justice to his memory, but we cannot refrain from taking the earliest opportunity of giving a slight outline of his long, active, and useful career. In so doing we pay, however imperfectly, the tribute which is due to one of our most respected fellow-citizens, who nobly sustained the credit of the profession to which his abilities were devoted, and deservedly acquired the friendship, esteem, and confidence of the large circle of eminent men with whom it brought him into constant and familiar intercourse.

Richard Taylor was born on the 18th of May, 1781, at Norwich. He was the second son (of a family of seven) of John Taylor, wool-comber, and Susan Cooke, and great-grandson of Dr. John Taylor, the author of the celebrated 'Hebrew Concordance.' His education was received at a day-school in Norwich, kept by the Rev. John Houghton, whom he describes as an excellent grammarian and a severe disciplinarian. Under this able tutor and his son, he made early and considerable progress in classical learning, and also acquired some knowledge of chemistry and other branches of natural philosophy. It seems to have been the wish of the master that his pupil should proceed to the High School of Glasgow (where he had himself received his education), and there qualify himself for the ministry; but other counsels prevailed, and, principally at the suggestion of Sir James Edward Smith, the founder of the Linnæan Society, and a very intimate friend of his parents, he was induced to adopt

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the profession of a printer—a profession to which he became ardently attached. On Sir James Smith's recommendation, he was apprenticed to Mr. Davis of Chancery Lane, London, a printer of eminence, from whose press issued many scientific works of importance. During this period of his life, his leisure hours seem to have been employed in the study not only of the classics, but also of the mediæval Latin and Italian authors, especially the poets, of whose writings he formed a curious collection. From these, his "old dumps" as he was wont to call them, he derived great pleasure to the last moments of his life. He also became a proficient scholar in French, Flemish, Anglo-Saxon and several of the kindred Teutonic dialects,—a proficiency which afterwards proved of eminent utility in his professional career, by far the greater number of the Anglo-Saxon works, and works connected with that branch of literature, published in London during the last forty years, having issued from his press.

On the expiration of his apprenticeship, he carried on business for a short time in Chancery Lane, in partnership with a Mr. Wilks; but on his birthday in the year 1803, at the age of twenty-two, he established himself, in partnership with his father, in Blackhorse Court, Fleet Street, from whence he soon after removed to Shoe Lane, and subsequently to Red Lion Court. His press speedily became the medium through which nearly all the more important works in scientific natural history were ushered into the world; and the careful accuracy by which all its productions were distinguished led to a rapid extension of its use. It was immediately adopted by the Linnæan Society; the Royal Society and many other learned bodies succeeded; individual members naturally followed the example of the Societies to which they belonged; and the same valuable qualities which had rendered it so acceptable to men of science were equally appreciated by those engaged in other pursuits. The beautiful editions of the Classics which proceeded from it, soon rendered his favourite device (the lamp receiving oil, with its motto of "Alere flammam") as familiar to all who had received a classical education in England as it had been from the beginning to the world of science. It would be tedious to enumerate even the more important of these works; but there is one in all respects so remarkable as to deserve especial mention. This is the facsimile of the Psalms from the Codex Alexandrinus, edited by the Rev. H. H. Baber, "at whose chambers in the British Museum," says Mr. Taylor in his Diary, under date of the 11th Nov. 1811, "I have collated the proofs of the first and second sheets with the Codex letter by letter, and I intend, if possible, to do the same for all the rest." A more striking proof could not be adduced of his strict attention to the accuracy of his

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press, and of his persevering devotion even to the minutest duties of his profession.

In the year 1807 he became a Fellow of the Linnæan Society, and at the anniversary of 1810 he was elected Under-Secretary, an office which he retained for nearly half a century, and in which he earned for himself the cordial esteem and good-will of every member of the Society. In his Diary, under date of the anniversary of 1849, he notes that he had "served with M'Leay, Bicheno, Dr. Boott, and Mr. Bennett, under the successive presidencies of the founder Sir J. E. Smith (the intimate and dear friend of my parents and my warm friend), of the Earl of Derby, the Duke of Somerset, and my excellent friend Dr. Stanley, Bishop of Norwich." To the names of the Presidents he might subsequently have added those of Mr. Brown and Mr. Bell; and he must have felt, though he was too modest himself to note it down, how highly he was esteemed by them all for his strict sense of honour, the amiability of his disposition, and his entire devotion to the interests of the Society.

Among the numerous other learned bodies of which he was a member, the Society of Antiquaries, the Astronomical Society, and the Philological were those in which he took the deepest interest. He also attached himself from its commencement to the British Association for the Advancement of Science, nearly all the meetings of which, while his health permitted, he regularly attended. At these pleasant gatherings of the scientific world, in the society of his numerous friends and of those whose names were most distinguished in science, many of the happiest days of his life were passed.

In 1822, as already stated, he joined Dr. Tilloch as editor of the 'Philosophical Magazine,' with which Dr. Thomson's 'Annals of Philosophy' were subsequently incorporated. In 1838 he established the 'Annals of Natural History,' and united with it, in 1841, Loudon and Charlesworth's 'Magazine of Natural History.' He subsequently (at the suggestion and with the assistance of some of the most eminent members of the British Association) issued several volumes of a work intended especially to contain papers of a high order of merit, chiefly translated, under the title of 'Taylor's Scientific Memoirs.' But his own principal literary labours were in the field of biblical and philological research. In 1829 he prepared a new edition of Horne Tooke's 'Diversions of Purley,' which he enriched with many valuable notes, and which he re-edited in 1840. In the same year (1840), Warton's 'History of English Poetry' having been placed in his hands by Mr. Tegg, the publisher, he contributed largely, in conjunction with his friends Sir F. Madden, Benjamin Thorpe, J. M. Kemble, and others,

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to improve the valuable edition published in 1824 by the late Mr. Richard Price.

For many years he represented the ward of Farringdon Without (in which his business premises were situated), in the Common Council of the City of London, and constantly paid strict attention to his representative duties. Of all the objects which came under his cognizance in this capacity there were none which interested him more deeply than questions connected with education. He took an active part in the foundation of the City of London School, and warmly promoted the establishment of University College and of the University of London. His politics were decidedly liberal; but his extended intercourse with the world, and the natural benevolence of his character, inclined him to listen with the most complete tolerance to the opinions of those who differed from him; and he reckoned among his attached friends many whose political opinions were strongly opposed to his own.

Early in the summer of 1852 his health gave way, and he found it necessary to withdraw from the excitement of active life. He settled down at Richmond, and once more gave himself up to Ovid, Virgil, and his old friends Paulus Manutius, Justus Lipsius, Ochinus, Fracastorius, &c. Increasing years brought increasing feebleness; and the severe weather of November last brought on an attack of bronchitis, of which he died suddenly on the 1st of December, in the seventy-eighth year of his age.—J. J. B.

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xvi. p. 542.]

June 10, 1858.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the formation of Continuous Tabular Masses of Stony Lava on steep slopes; with Remarks on the Mode of Origin of Mount Etna, and the Theory of 'Craters of Elevation.'" By Sir Charles Lyell, F.R.S. &c.

The question whether lava can consolidate on a steep slope, so as to form strata of stony and compact rock, inclined at angles of from 10° to more than 30°, has of late years acquired considerable importance, because geologists of high authority have affirmed that lavas which congeal on a declivity exceeding 5° or 6° are never continuous and solid, but are entirely composed of scoriaceous and fragmentary materials. From the law thus supposed to govern the consolidation of melted matter of volcanic origin, it has been logically inferred that all great volcanic mountains owe their conical form principally to upheaval or to a force acting from below and exerting an

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upward and outward pressure on beds originally horizontal or nearly horizontal. For in all such mountains there are found to exist some stony layers dipping at 10°, 15°, 25°, or even higher angles; and according to the assumed law, such an inclined position of the beds must have been acquired subsequently to their origin.

After giving a brief sketch of the controversy respecting "Craters of Elevation," the author describes the results of his recent visit (October, 1857) to Mount Etna, in company with Signor Gaetano G. Gemmellaro, and his discovery there of modern lavas, some of known date, which have formed continuous beds of compact stone on slopes of 15°, 36°, 38°, and, in the case of the lava of 1852, more than 40°. The thickness of these tabular layers varies from 1½ foot to 26 feet; and their planes of stratification are parallel to those of the overlying and underlying scoriæ which form part of the same currents. The most striking examples of this phenomenon were met with—1st, at Aci Reale; 2ndly, in the ravine called the Cava Grande near Milo, where a section of the lava of 1689 is obtained; 3rdly, in the precipice at the head of the Val di Calanna, in the lava of 1852–53; and 4thly, at a great height above the sea near the base of the Montagnuola.

Sir C. Lyell then alludes to the extraordinary changes which had taken place in the scenery of the Valley of Calanna and the Val del Bove since his former visit to Mount Etna in 1828—changes effected by the eruption of 1852–53, one of the greatest recorded in history. A brief account is given, extracted from contemporary narratives and illustrated by a map, compiled with the assistance of Dr. Giuseppe Gemmellaro, of the course taken in 1852–53 by various streams of lava, some of them six miles in length, flowing during nine successive months from the head of the Val del Bove to the suburbs of Zafarana and Milo. The present aspect of this lava-field, parts of it still hot and emitting vapour, and the numerous longitudinal ridges and furrows on its surface are described. As to the origin of these superficial inequalities, the author inquires whether they may be due to the flowing of lava in subterranean tunnels, or whether they be anticlinal and synclinal folds caused by fresh streams pouring over preceding and half-consolidated ones, so that these last may be bent and crumpled by the newly superimposed weight, like soft yielding ground on which a railway embankment has been made. The cascade of the lava of 1852, descending a precipitous declivity 500 feet high, called the Salto della Giumenta, and the stony character of the layers which encrust the steep slope at angles of more than 35° and even 45°, are commented upon. This lava has overflowed that of 1819, which congealed on the same precipice; and it is shown that in such cases the junction-lines separating two successive currents must be obliterated, the bottom scoriæ of the newer dovetailing into the upper scoriæ of the older current.

The structure of the nucleus of Etna, as exhibited in sections in the Val del Bove, is next treated of, and the doctrine of a double axis is deduced from the varying dip of the beds. The strata of trachyte and trachytic agglomerate in the Serra Giannicola seen at

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the base of the lofty precipice at the head of the Val del Bove are inclined at angles of 20° to 30° N.W., *i. e.* towards the present central axis of eruption. Other strata to the eastwards (as in the hill of Zoccolaro) dip in an opposite direction, or S.E., while, in a great part of the north and south escarpments of the Val del Bove, the beds dip N.E. or N., and S.E. or S. respectively. There is, therefore, a quâquâversal dip away from some point situated in the centre of the area called the Piano di Trifoglietto. Here a permanent axis of eruption may have existed for ages in the earlier history of Etna, for which the name of the axis of Trifoglietto is proposed, while the modern centre of eruption, that now in activity, may be called the axis of Mongibello. The two axes, which are three miles distant the one from the other, are illustrated by an ideal section through the whole of Etna, passing from west to east through the Val del Bove, or from Bronte to Zafarana. Touching the relative age of the two cones, it is suggested that a portion only of that of Mongibello may be newer than the cone of Trifoglietto. The latter, when it became dormant, was entirely overwhelmed and buried under the upper and more modern lavas of the greater cone. This doctrine of two centres, originally hinted at by the late Mario Gemmellaro, had been worked out (unknown to Sir C. Lyell at the time of his visit) by Baron Sartorius v. Waltershausen, and has been since supported in the fifth and sixth parts of his great work called "The Atlas of Etna" both by arguments founded on the quâquâversal dip of the beds as above explained, and by the convergence of a certain class of greenstone dikes towards the axis of Trifoglietto. Von Waltershausen has also shown that the superior lavas and volcanic formations crowning the precipices at the head of the Val del Bove, from the Serra Giannicola to the Rocca del Corvo, inclusive, are unconformable to the highly inclined beds in the lower half of the same precipice, the superior beds being horizontal, or, when inclined, dipping in such directions as would imply that they slope away from the higher parts of Mongibello.

According to Sir C. Lyell, the alleged discontinuity between the older and modern products of Etna is, in truth, only partial, and almost confined to that flank of the mountain, where its physical geography has been altered by three causes: 1st, the interference of the two foci of eruption (Trifoglietto and Mongibello); 2ndly, the truncation of the cone of Mongibello; and 3rdly, the formation of the Val del Bove. The truncation of the mountain here alluded to is proved by the remains of the upper portion of a cone, traceable at intervals around the borders of an elevated platform between 9000 and 10,000 feet high. These remains bear the same relation to the highest and active cone, nearly in the centre of the platform, which Somma bears to Vesuvius. The manner in which the north and south escarpments of the Val del Bove diminish in altitude as they trend eastward from the high platform, is appealed to as showing that the great lateral valley had no existence till after the time when Mongibello had attained its fullest development and height.

The double axis of Etna is then compared to the twofold axis of the island of Madeira, as inferred from observations made in 1854

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by M. Hartung and the author. In that island the principal chain of volcanic vents, running east and west, and 30 miles long, attains at one point a height of 6000 feet. Parallel to it, at the distance of two miles, a shorter and lower, secondary chain once existed, but was afterwards overflowed and buried to a great depth by lavas issuing from the higher and dominant chain. The space between the two axes, like the space which separated the two cones of Etna, has been filled up with lavas in part horizontal. On the north side of Madeira, as probably on the west side of Etna, where no secondary centre of eruption interfered with the slope of the volcanic formations, and where the order of their succession and superposition is uninterrupted, there occur, both in Madeira and Etna, deep crateriform valleys (the Curral and the Val del Bove) intersecting the products of the two axes of eruption.

In concluding this part of his memoir, Sir C. Lyell observes, that the admission of a double axis, as explained by him, is irreconcileable with the hypothesis of "craters of elevation;" for it implies that, in the cone-making process, the force of upheaval merely plays a subordinate part. One cone of eruption, he says, may envelope and bury an adjoining cone of eruption; but it is obviously impossible that one cone of upheaval should mantle round and overwhelm another cone of upheaval.

An attempt is then made to estimate the proportional amount of inclination which may be due to upheaval in those parts of the central nucleus of Etna where the dip is too great to be ascribed exclusively to the original steepness of the flanks of the cone. The highest dip seen by the author was in the rock of Musarra, where some of the strata, consisting of scoriæ with a few intercalated lavas, are inclined at 47°. Some masses of agglomerate and beds of lava in the Serra del Solfizio were also seen inclined at angles exceeding 40°. Some of these instances are believed to be exceptional and due to local disturbance; others may have an intimate connexion with the abundance of fissures, often of great width, filled with lava, for such *dikes* are much more frequent near the original centres of eruption than at points remote from them. The injection of so much liquid matter into countless rents may imply the gradual tumefaction and distension of the volcanic mass, and may have been attended by the tilting of the beds, causing them to slope away at steeper angles than before, from the axis of eruption. But instead of ascribing to this mechanical force, as many have done, nearly all, or about four-fifths of the whole dip, Sir C. Lyell considers that about one-fifth may, with more probability, be assigned as the effect of such movements.

The alleged parallelism and uniformity of thickness in the volcanic beds of the Val del Bove, when traced over wide areas, is next considered, and the author remarks that neither in the northern nor southern escarpments of the great valley, could he and his companion verify the existence of such parallelism. Examples of a marked deviation from it are given, both in cliffs seen from a distance, and in others which were closely inspected, even in cases where these last,

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when viewed from far off, appeared to contain regular and parallel strata.

The direction and position of the dikes in the Val del Bove is then spoken of, both in reference to the two ancient centres of eruption, and to the question of the altered inclination of the intersected beds. In regard to the arrangement also of the lateral cones of eruption, the question is entertained, whether they are disposed in linear zones, or are in some degree independent of the great centre of Mongibello.

The origin of the Val del Bove has been variously ascribed to engulfment, explosion, and aqueous erosion. Admitting the probable influence of the two first causes, the author calls attention to the positive evidence in favour of aqueous denudation afforded by the accumulation of alluvium in the low country at the eastern base of Etna between the Val del Bove and the sea. This rudely stratified deposit, 150 feet thick and several miles in length and breadth, contains at Giarre, Mangano, Riposto and other places, fragments, both rounded and angular, of all the rocks, ancient and modern, occurring in the escarpments of the Val del Bove, and it implies the continuance there for ages of powerful aqueous erosion. The alluvium of Giarre is therefore supposed to bear the same relation to the Val del Bove that the conglomerate of the Barranco de las Angustias bears to the Caldera of Palma in the Canaries; and those two crater-like valleys, as well as the Curral of Madeira, are believed to have been shaped out in great part by running water. But to render this possible, the suspension, for a long period, of the outpouring of lava on the eastern flank of Etna must be assumed.

The author fully coincides in the generally received opinion that the accessible parts of Etna are of subaërial origin, and refers to some fossil leaves presented to him by MM. Gravina and Tornabene, of Catania, as well as to others collected by himself *in situ*, from the volcanic tuffs of Fasano and Licatia, which have been determined by Prof. Heer to belong to terrestrial plants, of the genera Myrtle, Laurel, and Pistachio, now living in Sicily. These tuffs, together with the general mass of Etna, repose on marine strata of the newer Pliocene period, in which 150 species of shells, nearly nine-tenths of them identical with species now existing in the Mediterranean, have been found. A very modern marine breccia, with shells of living species extending to the height of thirty feet on the coast along the eastern base of Etna, was pointed out to the author by Signor G. G. Gemmellaro near Trezza, and in the Island of the Cyclops. The same formation has been traced together with lithodomous perforations by Dr. Carlo Gemmellaro and Baron v. Waltershausen along the sea-shore as far north as Taormina, beyond the volcanic region of Etna. From these and other data enlarged upon in the memoir, Sir C. Lyell concludes, first, that a very high antiquity must be assigned to the successive eruptions of Etna, each phase of its volcanic energy, as well as the excavation of the Val del Bove, having occupied a lapse of ages compared to which the historical period is brief and insignificant; and secondly, that the growth of the whole

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mountain must nevertheless be referred, geologically, to the more modern part of the latest Tertiary epoch.

"On some Thermo-dynamic Properties of Solids." By J. P. Joule, LL.D., F.R.S. &c.

A *résumé* of the greater part of this paper has already appeared in the 'Proceedings' for January 29, June 18, and November 26, 1857. The author has since examined the expansion by heat of wood cut across the grain, which, as well as that cut in the direction of the fibre, he finds to be increased by tension and decreased by moisture. When a sufficient quantity of water has been absorbed the expansibility by heat ceases, and wood is *contracted* in each direction by rise of temperature. Nevertheless, when wood, saturated with water, is weighed in water of different temperatures, the result shows cubical expansion of the substance of the wood by heat. The inference drawn by the author from these facts is, that the contraction of the dimensions of wet wood is owing to the action of heat in diminishing the force of capillary attraction, and that thus the walls of the minute cells and tubes of the woody structure are partially relieved from a force which thrusts them asunder, a small quantity of water exuding at the same time. In the case of wet wood which contracts by heat, he finds, in accordance with Professor Thomson's formula, that a rise of temperature is produced by the application of tension. In conformity with the deductions of the same philosopher, the author has also been able to detect experimentally the minute quantity of heat absorbed, in bending or twisting an elastic spring, arising from the diminution of the elastic force of metals with a rise of temperature.

"On the Thermal Effect of drawing out a Film of Liquid." By Professor William Thomson, F.R.S.

A very novel application of Carnot's cycle has just occurred to me in consequence of looking this morning into Waterston's paper on Capillary Attraction, in the January Number of the Philosophical Magazine. Let T be the contractile force of the surface (by which in Dr. Thomas Young's theory the resultant effect of cohesion on a liquid mass of varying form is represented), so that, if Π be the atmospheric pressure, the pressure of air within a bubble of the liquid of radius r, shall be $\frac{4T}{r} + \Pi$. Then if a bubble be blown from the end of a tube (as in blowing soap-bubbles), the work spent, per unit of augmentation of the area of one side of the film, will be equal to 2T.

Now since liquids stand to different heights in capillary tubes at different temperatures, and generally to less heights at the higher temperatures, T must vary, and in general decrease, as the temperature rises, for one and the same liquid. If T and T′ denote the values of the capillary tension at temperatures t and t' of our absolute scale, we shall have $2(T-T')$ of mechanical work gained, in allowing a bubble on the end of a tube to collapse so as to lose a

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unit of area at the temperature t and blowing it up again to its original dimensions after having raised its temperature to t'. If $t'-t$ be infinitely small, and be denoted by $\mathfrak{T}$, the gain of work may be expressed by

$$-\frac{2d\mathrm{T}}{dt}\times\mathfrak{T};$$

and by using Carnot's principle as modified for the Dynamical Theory, in the usual manner, we find that there must be an absorption of heat at the high temperature, and an evolution of heat at the low temperature; amounting to quantities differing from one another by

$$\frac{1}{\mathrm{J}}\times\frac{-2d\mathrm{T}}{dt}\times\mathfrak{T},$$

and each infinitely nearly equal to the mechanical equivalent of this difference, divided by Carnot's function, which is $\frac{\mathrm{J}}{t}$, if the temperature is measured on our absolute scale. Hence if a film such as a soap-bubble be enlarged, its area being augmented in the ratio of 1 to m, it experiences a cooling effect, to an amount calculable by finding the lowering of temperature produced by removing a quantity of heat equal to

$$m\frac{t}{\mathrm{J}}\times\frac{-d\mathrm{T}}{dt},$$

from an equal mass of liquid unchanged in form.

For water T=2·96 gr. per lineal inch.

Work per square inch spent in drawing out a film =5·92, say 6 grains, $\frac{d\mathrm{T}}{dt}=\frac{1}{550}\mathrm{T}$, or thereabouts.

Suppose $\frac{t'}{\mathrm{J}}=\frac{300}{1390\times12^2}$, then the quantity of heat to be removed, to produce the cooling effect, per square inch of surface of augmentation of film will be $\frac{1}{5100}$. Suppose, then, 1 grain of water to be drawn out to a film of 16 square inches, the cooling effect will be $\frac{16}{5100}$ of a degree Centigrade, or about $\frac{1^\circ}{320}$. The work spent in drawing it out is $16\times6=96$ grains and is equivalent to a heating effect of $\frac{96}{12\times1390}=\frac{1}{174}$. Hence the total energy (reckoned in heat) of the matter is increased $\frac{1}{174}+\frac{1}{320}$ of a degree Centigrade, when it is drawn out to 16 square inches.

"On the Logocyclic Curve, and the geometrical origin of Logarithms." By the Rev. J. Booth, LL.D., F.R.S.

June 17.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"Description of some Remains of a Gigantic Land-Lizard (*Megalania prisca*, Ow.) from Australia." By Prof. Richard Owen, F.R.S.

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"Contributions towards the History of the Diamides; Cyanate and Sulphocyanide of Phenyle." By A. W. Hofmann, Ph.D., F.R.S.

About ten years* ago, when engaged in the study of aniline, I discovered two beautiful crystalline compounds, carbanilide and sulphocarbanilide, which can be produced by a variety of processes. The former is best prepared by the action of phosgene-gas on aniline, while the latter is most readily and most abundantly procured by the action of bisulphide of carbon on aniline. The composition and the constitution of these bodies is indicated by the formulæ—

$$\text{Carbanilide} \ldots\ldots C_{26} H_{12} N_2 O_2 = \left.\begin{matrix}(C_2\ O_2)'' \\ (C_{12} H_5)_2 \\ H_2\end{matrix}\right\} N_2,$$

$$\text{Sulphocarbanilide} \ldots C_{26} H_{12} N_2 S_2 = \left.\begin{matrix}(C_2\ S_2)'' \\ (C_{12} H_5)_2 \\ H_2\end{matrix}\right\} N_2.$$

They may be viewed as derived from two molecules of ammonia (diammonia) in which two equivalents of hydrogen are replaced by two molecules of phenyle, and two other equivalents by the biatomic molecules $C_2\ O_2$ and $C_2\ S_2$.

The two substances in question, as far as their formulæ are involved, obviously correspond to urea and sulphocyanide of ammonium:—

$$\text{Urea} \ldots\ldots\ldots\ldots\ldots\ldots C_2 H_4 N_2 O_2 = \left.\begin{matrix}(C_2 O_2)'' \\ H_2 \\ H_2\end{matrix}\right\} N_2,$$

$$\text{Sulphocyanide of ammonium} \ldots C_2 H_4 N_2 S_2 = \left.\begin{matrix}(C_2 S_2)'' \\ H_2 \\ H_2\end{matrix}\right\} N_2.$$

In their formation likewise a certain analogy with urea and sulphocyanide of ammonium may be recognized; for recent experiments have proved that urea is actually produced by the action of phosgene-gas on ammonia, while the formation of sulphocyanide of ammonium by means of ammonia and bisulphide of carbon is a long established fact. The analogy, however, seems to disappear altogether, if the chemical nature of the four bodies be compared, for while urea exhibits the deportment of a base, and the saline character of sulphocyanide of ammonium is so well defined, carbanilide and sulphocarbanilide are apparently perfectly indifferent substances.

Nevertheless, on considering the difference of the chemical properties of urea and sulphocyanide of ammonium, and on recollecting that the saline constitution of urea is much more hidden than that of sulphocyanide of ammonium, it appeared worth while to try whether the action of powerful agents would not reveal a similar, if I may use the term, saline construction in carbanilide and sulphocarbanilide. Experiment has realized this anticipation.

* Journal of the Chemical Society, vol. ii. 36.

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In the conception of the above view, I have endeavoured to split the two bodies in question according to the equations—

$$\underbrace{C_{26}H_{12}N_2O_2}_{\text{Carbanilide, Diphenyle-carbamide.}} = \underbrace{C_{12}H_7N}_{\text{Phenylamine.}} + \underbrace{C_{14}H_5NO_2}_{\text{Cyanate of phenyle.}},$$

and

$$\underbrace{C_{26}H_{12}N_2S_2}_{\text{Sulphocarbanilide, Diphenyle-sulphocarbamide.}} = \underbrace{C_{12}H_7N}_{\text{Phenylamine}} + \underbrace{C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}}$$

suggested by analogous changes of urea and sulphocyanide of ammonium :—

$$\underbrace{C_2H_4N_2O_2}_{\text{Urea.}} = H_3N + \underbrace{C_2HNO_2}_{\text{Cyanic acid.}},$$

$$\underbrace{C_2H_4N_2S_2}_{\text{Sulphocyanide of ammonium.}} = H_3N + \underbrace{C_2HNS_2}_{\text{Hydrosulphocyanic acid.}}$$

These reactions succeed without much difficulty. On submitting carbanilide and sulphocarbanilide to the action of agents capable of fixing aniline (anhydrous phosphoric acid, chloride of zinc, and even hydrochloric acid gas), the former yields *cyanate of phenyle*, a substance which I discovered many years ago among the products of decomposition of oxamelanile*, while the latter furnishes a remarkable body, *sulphocyanide of phenyle*, which had not been previously obtained.

The general features of cyanate of phenyle having been delineated in a former memoir, I have for the present been chiefly engaged with the examination of sulphocyanide of phenyle. This body, which is readily obtained in a state of absolute purity by rectification over anhydrous phosphoric acid, is a colourless transparent liquid of 1·135 density at 15°·5, and of a perfectly constant boiling-point at 222° C. under a pressure of 0^{m}·762. The odour is aromatic and pungent; it distantly resembles that of mustard; the body in question is in fact the mustard oil of the phenyle-series.

Mustard oil, sulphocyanide of allyle $C_8H_5NS_2 = C_6H_5, C_2NS_2$.

Sulphocyanide of phenyle ..... $C_{14}H_5NS_2 = C_{12}H_5, C_2NS_2$.

Sulphocyanide of phenyle may be distilled with water, and even with hydrochloric acid, without undergoing any change. The alkalies, on the other hand, decompose it. Boiled with an alcoholic solution of potassa, it is first converted into sulphocarbanilide, and ultimately into carbanilide.

* Journal of the Chemical Society, vol. ii. 313.

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$$\underbrace{2C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}} + 4KO + 2HO = 2KS + K_2C_2O_6 + \underbrace{C_{26}H_{12}N_2S_2}_{\text{Sulphocarbanilide.}}.$$

$$\underbrace{2C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}} + 6KO + 2HO = 4KS + K_2C_2O_6 + \underbrace{C_{26}H_{12}N_2O_2}_{\text{Carbanilide.}}$$

When gently warmed with phenylamine, sulphocyanide of phenyle is instantaneously converted into sulphocarbanilide,—

$$\underbrace{C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}} + \underbrace{C_{12}H_7N}_{\text{Phenyl-amine.}} = \underbrace{C_{26}H_{12}N_2S_2}_{\text{Sulphocarb-anilide.}}$$

A similar reaction takes place with ammonia. An alcoholic solution of ammonia, when gently warmed with sulphocyanide of phenyle, readily solidifies into a crystalline compound, which may be obtained in beautiful needles by crystallization from boiling water.

The new body is formed according to the equation

$$\underbrace{C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}} + H_3N = \underbrace{C_{14}H_8N_2S_2}_{\text{Sulphophenyl-carbamide.}}$$

This substance is the thiosinamine of the phenyle-series; like the latter, it possesses the characters of a weak base. I have not been able to obtain saline compounds with hydrochloric and sulphuric acids. It forms, however, compounds with nitrate of silver and bichloride of platinum. The latter has the usual composition, viz.—

$$C_{14}H_8N_2S_2, HCl, PtCl_2.$$

Boiled with nitrate of silver, the new compound loses its sulphur, which is replaced by oxygen, phenylcarbamide, $C_{14}H_8N_2O_2$, being produced, a substance which I described many years ago. Sulphocyanide of phenyle is acted upon by a great number of ammonias, with formation of bodies the composition of which is sufficiently pointed out by theory.

The mode of producing cyanate and sulphocyanide of phenyle, which I have described in the preceding paragraphs, deserves some notice, since the usual processes suggested by the experience in the methyle-, ethyle- and amyle-series, such as distillation of sulphophenylates with cyanates and sulphocyanides, have altogether failed in producing the desired result. The same reaction may be of

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course applied to tolylamine, cumylamine, naphthylamine, and all primary monamines.

"Action of Bibromide of Ethylene upon Aniline." By A. W. Hofmann, Ph.D., F.R.S.

While engaged in some experiments on the action of bibromide of ethylene on ammonia, a short account of which I have lately communicated to the Royal Society*, I induced Mr. Henry Bassett, then working in my laboratory, to study the deportment of the same bromide with aniline, a characteristic representative of the class of primary monamines. In the following pages I propose to submit to the Society Mr. Bassett's observations, together with the results of a series of experiments which I carried out myself after Mr. Bassett by circumstances had been prevented from a further continuation of the inquiry.

A mixture of 1 volume of the bibromide of ethylene and 2 volumes of aniline, when exposed to the temperature of boiling water for an hour or two, solidifies into a crystalline mass of more or less solidity. This mass is chiefly hydrobromate of aniline; it contains, however, in addition, three new organic bases, partly free, partly in the form of hydrobromates. These substances are formed in very different quantities,—a beautiful crystalline body, difficultly soluble in alcohol, being invariably the chief product of the reaction, while the two other bases, the one solid but extremely soluble in alcohol, the other likewise solid but quite insoluble in this liquid, are found to be present in much smaller proportions.

The preparation, in a state of purity, of the principal product of the reaction presents no difficulty. The solid mass obtained by digesting bibromide of ethylene and aniline in the stated proportions is mixed with water, and submitted to distillation, when any bibromide left unchanged, together with some unaltered aniline, passes over. The residuary liquid is then mixed with a strong solution of potassa, which separates all the bases existing as hydrobromates in the form of a semi-solid resin. This is washed with water and then again submitted to distillation with water, when, together with more or less water, an additional quantity of aniline distils. The residuary mass, when treated with boiling (methylated) spirit, leaves the insoluble base as a white, flour-like powder, while the other two bases dissolve. On cooling, the solution deposits a beautiful crystallization of white needles, while the more soluble base remains dissolved in the spirit. The crystals are rather difficultly soluble in alcohol; two or three crystallizations from this solvent render them absolutely pure.

Thus obtained, the new base, for which, in accordance with the results of analysis, I propose the name *ethylene-phenylamine*, is a snow-white, inodorous and tasteless crystalline compound, of nacreous lustre, insoluble in water, soluble in boiling, less so in cold alcohol, soluble

* Proceedings of the Royal Society, vol. ix. page 150.

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in ether. The solutions are without action on vegetable colours. The substance dissolves readily in hydrochloric, sulphuric and nitric acids, especially on gently heating the liquids, which on cooling deposit well crystallized saline compounds. The hydrochlorate yields yellow precipitates with bichloride of platinum and terchloride of gold. When exposed to the action of heat, ethylene-phenylamine fuses at 148° C.; at a temperature approaching 300° it begins to boil and to distil, the larger portion undergoing decomposition. Among the products of decomposition which are not yet sufficiently examined, considerable quantities of aniline make their appearance. The results obtained in the analysis of ethylene-phenylamine lead to the formula

$$C_{16}H_9N$$

as the simplest molecular expression for this compound.

This formula is confirmed by the analysis of the hydrochlorate and of the platinum-salt, the preparation of which, on account of their instability, requires some management.

These salts contain respectively

Hydrochlorate.....$C_{16}H_9N$, HCl.
Platinum-salt .....$C_{16}H_9N$, HCl, $PtCl_2$.

The reaction which gives rise to ethylene-phenylamine is expressed by the following equation:—

$$\underbrace{2C_{12}H_7N}_{\text{Phenylamine.}} + \underbrace{C_4H_4Br_2}_{\substack{\text{Bibromide}\\ \text{of ethylene.}}} = \underbrace{C_{12}H_7N, HBr}_{\substack{\text{Hydrobromate}\\ \text{of phenylamine.}}} + \underbrace{C_{16}H_9N, HBr}_{\substack{\text{Hydrobromate of}\\ \text{ethylene-phenylamine.}}}.$$

What is the constitution of this new base? This question could not be answered without further experiments, on account of the twofold nature of bibromide of ethylene. In many cases this remarkable compound exhibits the character of the hydrobromic ether of a biacid ethylene-alcohol, of $(C_4H_4)''Br_2$, whilst in the majority of reactions it splits into hydrobromic acid and the bromide C_4H_3Br, which might be considered as the hydrobromic ether of a monacid alcohol, $C_4H_4O_2$, homologous to allylic alcohol. It remained therefore uncertain whether the new basic compound retained the original molecule $(C_4H_4)''$ replacing 2 equivs. of hydrogen, or the modified molecule C_4H_3 replacing 1 equiv. of hydrogen. In other words, it had to be established by further experiments, whether the base was

$$\left.\begin{matrix}(C_4H_4)''\\ C_{12}H_5\end{matrix}\right\} N = C_{16}H_9N, \text{ or}$$

$$\left.\begin{matrix}C_4H_3\\ C_{12}H_5\\ H\end{matrix}\right\} N = C_{16}H_9N.$$

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The deportment of the substance with iodide of methyle and ethyle, which immediately will be mentioned somewhat more in detail, has decided in favour of the former view, and in accordance with it the name of the substance has been selected.

It deserves to be noticed, that there are already two other bases known which have exactly the same composition, the one obtained by M. Natanson in the reaction of bichloride of ethylene upon aniline, and described by him as *acetylaniline*, the other discovered by M. Dusart among the derivatives of nitronaphtaline and designated as phtalidine. It is only necessary superficially to glance at the description of these bodies in order to see that they are essentially different from ethylene-phenylamine. The constitution of acetylaniline and phtalidine has not been experimentally fixed. It is probable that Natanson's base contains the molecule $C_4 H_3$ formerly called acetyle, but for which the more appropriate term vinyle has lately been proposed, while phtalidine probably derives from the hydrocarbon styrole or an isomeric body, so that the difference in the constitution of the three bodies would be expressed in the following formulæ :—

Phtalidine / Styrylamine (?) $\left\{ \begin{matrix} C_{16} H_7 \\ H \\ H \end{matrix} \right\}$ N.

Acetylaniline / Vinyl-phenylamine $\left\{ \begin{matrix} C_4 H_3 \\ C_{12} H_5 \\ H \end{matrix} \right\}$ N.

Ethylene-phenylamine $\left\{ \begin{matrix} (C_4 H_4)'' \\ C_{12} H_5 \end{matrix} \right\}$ N.

I have already mentioned that the degree of substitution of ethylene-phenylamine was fixed by the deportment of this base with iodide of methyle and ethyle, bibromide of ethylene exerting no longer any influence upon it, even by protracted contact, at temperatures varying from 100° to 150° C.

A mixture of ethylene-phenylamine and iodide of methyle, on the other hand, when exposed for some hours to the temperature of boiling water, solidifies to a resinous mass, floating, together with a portion of unchanged base, in the excess of the iodide. Distillation with water separates the excess of iodide of methyle ; and washing with cold water until the filtrate is no longer precipitated by an alkali removes any hydriodate of ethylene-phenylamine formed during the distillation. Lastly, by repeated crystallization of the resinous residue from boiling water, to which a small quantity of spirit may be added in the later stages (separation from ethylene-phenylamine), a perfectly crystalline, slightly yellowish iodine-compound is obtained, which may be dried without decomposition at 100°.

On analysis, this iodine-compound was found to have the remark-

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able composition

$$C_{34}H_{21}N_2I = \left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\} C_2H_3I.$$

Treated with oxide of silver, the solution of the iodide yields a powerfully alkaline liquid, possessing all the characters of the class of bodies of which hydrated oxide of tetrethylammonium is the type. On adding hydrochloric acid and bichloride of platinum, this liquid furnishes a pale yellow amorphous platinum-salt containing

$$C_{34}H_{21}N_2Cl, PtCl_2 = \left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\} C_2H_3Cl, PtCl_2.$$

A repetition of this experiment in the ethyle-series has given perfectly similar results. On account of the less powerful action of iodide of ethyle, the reaction requires longer digestion. The iodide formed is less soluble in boiling water than the corresponding methyle-compound, and therefore more difficult to separate from any ethylene-phenylamine which may have remained unchanged. When pure, the new iodide is a yellowish white substance crystallizing in needles. It fuses in the water-bath without decomposition to a yellow oil, which solidifies on cooling into a brittle crystalline mass.

On analysis, numbers were obtained corroborating in every respect the results furnished by the methyle-series. The iodide contains

$$C_{36}H_{23}N_2I = \left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\} C_4H_5I.$$

Like the methyle-compound, it is readily decomposed by oxide of silver; and the powerfully alkaline solution yields, with hydrochloric acid and bichloride of platinum, a salt of exactly the same appearance as the salt of the methyle-series. This platinum-salt was found to contain

$$C_{36}H_{23}N_2Cl, PtCl_2 = \left.\begin{matrix}C_{16}H_9N\\C_{16}H_9N\end{matrix}\right\} C_4H_5Cl, PtCl_2.$$

The action of iodide of methyle and ethyle upon ethylene-phenylamine, although different from what might have been anticipated, nevertheless appears to fix in an unequivocal manner the state of substitution of this base. It is obvious that ethylene-phenylamine no longer contains any replaceable hydrogen, and consequently that the molecule $(C_4H_4)''$, equivalent to H_2 as such, has been assimilated by the aniline.

But how is the composition of the bodies formed by the action of iodide of methyle and ethyle to be interpreted? Are they simply compounds of the alcohol-iodides with 2 equivalents of ethylene-phenylamine, analogous to the salts produced by the union of 1 equiv. iodide of mercury with 2 equivs. of ammonia?

Does not the existence of these bodies involve a further considera-

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tion of the formula which has been assigned to ethylene-phenylamine? Does the formula $C_{16}H_9N$ actually represent the molecule of this body, or is it not more correct to double that expression and to consider the formula $C_{32}H_{18}N_2$ as a more appropriate representation of this molecule? Ethylene-phenylamine would then be derived from 2 equivalents of ammonia, it would be a diamine, and the hydrochlorate and the platinum-compounds would appear in the light of diammonium-compounds.

Diethylene-diphenyl-diamine $\left.\begin{matrix}(C_4H_4)_2''\\(C_{12}H_5)_2\end{matrix}\right\}N_2.$

Bichloride..... $\left.\begin{matrix}(C_4H_4)''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2Cl_2.$

Platinum-salt..... $\left.\begin{matrix}(C_4H_4)_2''\\(C_{12}H_5)_2\\H_2\end{matrix}\right\}N_2Cl_2, 2PtCl_2.$

At the first glance it certainly appears strange that a molecule capable of assimilating 2 equivs. of hydrochloric acid should unite only with 1 equiv. of iodide of methyle or ethyle, well established members of the hydrochloric type. But this deportment after all is not without parallelism.

The expression

$$C_{20}H_{12}NO_2,$$

originally established for quinine by Liebig, supported as it was by the analysis of numerous salts of the formula

$$C_{20}H_{12}NO_2, HX,$$

and especially by that of a platinum-compound,

$$C_{20}H_{12}NO_2, HCl, PtCl_2, aq,$$

was universally adopted by chemists.

A few quinine-salts of the formula

$$2(C_{20}H_{12}NO_2), HX$$

were considered as anomalous, as basic compounds; and it was not until the methylic and ethylic derivatives of quinine,

$$2C_{20}H_{12}NO_2, C_2H_3I \quad \text{and}$$
$$2C_{20}H_{12}NO_2, C_4H_5I,$$

had been discovered that chemists began to consider the formula

$$C_{40}H_{24}N_2O_4$$

as a more appropriate expression for the molecule of quinine.

Probably further examination of the salts of ethylene-phenylamine

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—I retain this name for the present—will furnish saline compounds corresponding to the methyle- and ethyle-derivatives, showing that this base, like quinine, is capable of forming two groups of salts.

It deserves to be noticed that the diammonic nature of ethylene-phenylamine is also strongly marked by its deportment under the influence of heat; for while all the monammonic basic derivatives of aniline are volatile without decomposition, ethylene-phenylamine, when submitted to distillation, is destroyed with reproduction of aniline, like the well-established diamines belonging to this group, melaniline, formyl-diphenylamine, &c.

In describing the preparation of ethylene-phenylamine, it has been mentioned that the action of bibromide of ethylene on aniline gives rise at the same time to two other basic compounds. These substances, which are formed in smaller quantity, differ in a very marked manner from the principal product of the reactions. Their study is not yet completed, but it may even now be stated that they have the same composition as ethylene-phenylamine itself. One of these substances, remarkable for its solubility in spirit, is capable of being converted into ethylene-phenylamine by a simple molecular change. The relation in which these three isomeric bodies stand to each other is not yet finally fixed by experiment. The idea suggests itself that it may possibly be represented by the formulæ—

Soluble base.....	$C_{16}\,H_9\,N$.
Ethylene-phenylamine	$C_{32}\,H_{18}\,N_2$.
Insoluble base.....	$C_{48}\,H_{27}\,N_3$.

"On Curves of the Third Order." By the Rev. George Salmon, of Trinity College, Dublin.

The author remarks that his paper was intended as supplementary to Mr. Cayley's Memoir "On Curves of the Third Order" (Philosophical Transactions, 1857, p. 415). He establishes in the place of Mr. Cayley's equation, p. 442, a fundamental identical equation, which is as follows, viz. if substituting in the cubic U, $x+\lambda x'$, $y+\lambda y'$, $z+\lambda z'$ for x, y, z, the result is

$$U+3\lambda S+3\lambda^2 P+\lambda^3 U';$$

so that S and P are the polar conic and polar line of (x', y', z'), with respect to the cubic, viz.

$$3S=x'\frac{dU}{dx}+y'\frac{dU}{dy}+z'\frac{dU}{dz};\quad 3P=x\frac{dU'}{dx'}+y\frac{dU'}{dy'}+z\frac{dU'}{dz'};$$

and if making the same substitution in the Hessian H, the result is

$$H+3\lambda\Sigma+3\lambda^2\Pi+\lambda^3 H',$$

so that Σ and Π are the polar conic and polar line of the Hessian—then the identical equation in question is

$$3(S\Pi-\Sigma P)=H'U-HU'.$$

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And it follows that when (x', y', z') is a point on the cubic, the equation $U=0$ of the cubic may be written in the form

$$S\Pi - \Sigma P = 0,$$

an equation which is the basis of the subsequent investigations of the paper. The author refers to a communication to him by Mr. Cayley, of an investigation of the equation of the conic passing through five consecutive points of the cubic, in the case where the equation of the cubic is presented in the canonical form $x^3+y^3+z^3+6lxyz=0$, and he shows that by the help of the above mentioned identity, the investigation can be effected with equal facility when the equation of the cubic is presented in the general form; and he establishes various geometrical theorems in relation to the conic in question. Finally, the author considers an entirely new question in the theory of cubics, viz. the determination of the points of a cubic, through which it is possible to draw an infinity of cubics having a nine-point contact, or complete osculation, with the given cubic. It is shown that the points in question are those which are their own third tangentials, and this suggests the consideration of the new canonical form, $x^2y+y^2z+z^2x+2mxyz=0$, of the equation of the cubic; this inquiry, however, is not pursued in the paper.

GEOLOGICAL SOCIETY.

[Continued from vol. xvi. p. 478.]

December 15, 1858.—Prof. J. Phillips, President, in the Chair.

The following communications were read:—

1. "On the Succession of Rocks in the Northern Highlands." By John Miller, Esq. Communicated by Sir R. I. Murchison, V.P.G.S.

Mr. Miller in this communication explained the history of our knowledge of the geology of this district; and, having given in detail an examination that he made of the coast last autumn, he drew particular attention to the faithful and comprehensive descriptions of the Old Red district by Sedgwick and Murchison in former years, and showed that his own observations quite coincide with the results of Sir Roderick Murchison's late correlation of the Gneissic, Cambrian, Silurian, and Old Red strata of the coasts of Sutherland, Ross-shire, and Caithness.

In conclusion, Mr. Miller pointed out that the Durness Limestone and the fossiliferous beds of Caithness were still open fields for careful and energetic explorers.

2. "On the Geological Structure of the North of Scotland. Part III. The Sandstones of Morayshire, containing Reptilian remains, shown to belong to the Uppermost division of the Old Red Sandstone." By Sir Roderick I. Murchison, F.R.S., D.C.L., V.P.G.S., &c.

Referring to his previous memoir for an account of the triple

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division of the Old Red Sandstone of Caithness and the Orkney Islands, the author showed how the chief member of the group in those tracts diminished in its range southwards into Ross-shire, and how, when traceable through Inverness and Nairn, it was scarcely to be recognized in Morayshire, but reappeared with its characteristic ichthyolites in Banffshire (Dipple, Tynet, and Gamrie).

He then prefaced his description of the ascending order of the strata belonging to this group in Morayshire by a sketch of the successive labours of geologists in that district; pointing out how in 1828 the sandstones and cornstones of this tract had been shown by Professor Sedgwick and himself to constitute, together with the inferior Red Sandstone and Conglomerate, one natural geological assemblage; that in 1839 the late Dr. Malcomeson made the important additional discovery of fossil fishes, in conjunction with Lady Gordon Cumming, and also read a valuable memoir on the structure of the tract, before the Geological Society, of which, to his, the author's regret, an abstract only had been published. (Proc. Geol. Soc. vol. iii. p. 141.)

Sir Roderick revisited the district in the autumn of 1840, and made sections in the environs of Forres and Elgin. Subsequently Mr. P. Duff, of Elgin, published a 'Sketch of the Geology of Moray,' with illustrative plates of fossil fishes, sections, and a geological map by Mr. John Martin; and afterwards Mr. Alexander Robertson threw much light upon the structure of the district, particularly as regarded deposits younger than those under consideration.

All these writers, as well as Sedgwick and himself, had grouped the yellow and whitish yellow sandstones of Elgin with the Old Red Sandstone; but the discovery in them of the curious small reptile, the *Telerpeton Elginense*, described by Mantell in 1851, from a specimen in Mr. P. Duff's collection, first occasioned doubts to arise respecting the age of the deposit. Still the sections by Capt. Brickenden, who sent that reptile up to London, proved that it had been found in a sandstone which dipped under 'Cornstone,' and which passed downwards into the Old Red series. Capt. Brickenden also sent to London natural impressions of the foot-prints of an apparently reptilian animal in a slab of similar sandstone, from the coast-ridge extending from Burgh Head to Lossiemouth (Cummingstone).

Although adhering to his original view respecting the age of the sandstones, Sir R. Murchison could not avoid having misgivings and doubts, in common with many geologists, on account of the high grade of reptile to which the *Telerpeton* belonged; and hence he revisited the tract, examining the critical points, in company with his friend the Rev. G. Gordon, to whose zealous labours he owned himself to be greatly indebted.

In looking through the collections in the public museum of Elgin and of Mr. P. Duff, he was much struck with the appearance of several undescribed fossils, apparently belonging to Reptiles, which by the liberality of their possessors, were, at his request, sent up for inspection to the Museum of Practical Geology. He was also much astonished at the state of preservation of a large bone (*ischium*),

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apparently belonging to a reptile, found by Mr. Martin in the same sandstone-quarries of Lossiemouth, in which the scales or scutes of the *Stagonolepis*, described as belonging to a fish by Agassiz, had been found. On visiting these quarries, Mr. G. Gordon and himself fortunately discovered other bones of the same animal; and these, having been compared with the remains in the Elgin collections, have enabled Professor Huxley to decide that, with the exception of the *Telerpeton*, all these casts, scales, and bones belong to the Reptile *Stagonolepis Robertsoni*.

Sir Roderick, having visited the quarries in the Coast-ridge, from which slabs with impressions of reptilian footmarks had long been obtained, induced Mr. G. Gordon to transmit a variety of these, which are now in the Museum of Practical Geology; and of which some were exhibited at the Meeting.

After reviewing the whole succession of strata from the edge of the crystalline rocks in the interior to the bold cliffs on the sea-coast, the author has satisfied himself that the reptile-bearing sandstones must be considered to form the uppermost portion of the Old Red Sandstone, or Devonian group,—the following being among the chief reasons for his adherence to this view.

1st. That these sandstones have everywhere the same strike and dip as the inferior red sandstones containing *Holoptychii* and other Old Red Ichthyolites, there being a perfect conformity between the two rocks, and a gradual passage from the one into the other. 2ndly. That the yellow and light colours of the upper band are seen in natural sections to occur and alternate with red and green sandstones, marls, and conglomerates low down in the ichthyolitic series. 3rdly. That, whilst the concretionary limestones called "Cornstones" are seen amidst some of the lowest red and green conglomerates, they reappear in a younger and broader zone at Elgin, and re-occur above the Telerpeton-sandstone of Spynie Hill, and above the Stagonolepis-sandstone of Lossiemouth; thus binding the whole into one natural physical group. 4thly. That, whilst the small patches of so-called "Wealden" or Oolitic strata, described by Mr. Robertson and others as occurring in this district, are wholly unconformable to, and rest upon, the eroded surfaces of all the rocks under consideration, so it was shown that none of the Oolitic or Liassic rocks of the opposite side of the Moray Frith, or those of Brora, Dunrobin, Ethie, &c., which are charged with Oolitic and Liassic remains, resemble the reptiliferous sandstones and "Cornstones" of Elgin, or their repetitions in the Coast-ridge, that extend from Burgh Head to Lossiemouth.

Fully aware of the great difficulty of determining the exact boundary-line between the Uppermost Devonian and Lowest Carboniferous strata, and knowing that they pass into each other in many countries, the author stated that no one could dogmatically assert that the reptile-bearing sandstones might not, by future researches, be proved to form the commencement of the younger era.

Sir Roderick concluded by stating that the conversion of the

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Stagonolepis into a reptile of high organization, though of nondescript characters, did not interfere with his long-cherished opinion—founded on acknowledged facts—as to the progressive succession of great classes of animals, and that, inasmuch as the earliest Trilobite of the invertebrate Lower Silurian era was as wonderfully organized as any living Crustacean, so it did not unsettle his belief to find that the earliest reptiles yet recognized, the *Stagonolepis* and *Telerpeton*, pertained to a high order of that class.

3. "On the *Stagonolepis Robertsoni* of the Elgin Sandstones; and on the Foot-marks in the Sandstones of Cummingstone." By Thomas H. Huxley, F.R.S., F.G.S., Professor of Nat. Hist., Government School of Mines.

The unquestionable remains of *Stagonolepis Robertsoni* which have hitherto been obtained consist partly of bones and dermal scutes, and partly of the natural casts of such parts. The former have been obtained only at Lossiemouth, and are comparatively few in number; the numerous natural casts, on the other hand, have all been procured at the Findrassie Quarry, in which no bones or scutes, in their original condition, have been discovered.

The considerable series of remains exhibited to the Society did not embrace all those which had been subjected to examination, but contained only a selection of those more characteristic parts upon which the conclusions of the author of the paper, respecting the structure and affinities of *Stagonolepis*, are based.

They were—1. Dermal scutes; 2. Vertebræ; 3. Ribs; 4. Bones of the extremities; 5. Bones of the pectoral arch; and 6. A natural cast of a mandible with teeth. The dermal scutes are all characterized by an anterior smooth facet, overlapped by the preceding scute, and by the peculiar sculpture of their outer surface, which exhibits deep, distinct, round or oval pits, so arranged as to appear to radiate from a common centre. Of these scutes there are two kinds, the flat and the angulated. By a careful comparison with the dermal armour of ancient and modern crocodilian reptiles, it was shown that every peculiarity of the scutes of *Stagonolepis* could find its parallel in those of *Crocodilus* or *Teleosaurus*,—the flat scutes resembling the ventral armour of the latter, the angulated scutes the dorsal armour of the former genus.

An unexpected verification of the justice of this determination was furnished by a natural cast of a considerable portion of the caudal region of *Stagonolepis*, consisting of no less than seven vertebræ, enclosed within the corresponding series of dermal scutes. Of these, the dorsal set were angulated; the ventral, flat.

It would appear that the anterior dorsal scutes attained a very considerable thickness, while the posterior scutes were widest, attaining more than five inches in breadth in some instances. The vertebræ described were all studied from natural casts, and belonged to the caudal, sacral, and anterior-dorsal series. These vertebræ are, in their leading features, similar to those of Teleosau-

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rians,—the obliquity of the articular faces of the centra, so characteristic of the vertebræ of *Stagonolepis*, being, as the author of the paper pointed out, a very common character of Teleosaurian, and even of modern Crocodilian, vertebræ. Of the sacral vertebræ, only a natural cast of the posterior face of the second had been obtained; but it was sufficient to demonstrate the wholly crocodilian characters of this region in *Stagonolepis*.

The dorsal vertebræ present a remarkable peculiarity in the strong upward, outward, and backward inclination of the transverse processes, and in the size of the facet for the head of the rib. The vertebra thus acquires a Dinosaurian character; but no great weight was attached to this circumstance, as the amount of upward inclination of the transverse processes of the anterior dorsal vertebræ varies greatly in both *Crocodilia* and *Enaliosauria*.

The ribs have well-marked and distinct capitula and tubercula; and the scapula is extremely like that of a crocodile. The femur, though somewhat thick in proportion to its length; and, though its articular extremities present such a peculiarly eroded appearance as to lead to the belief that they were covered with thick cartilaginous epiphyses, is also completely crocodilian in its characters.

The natural cast of the mandible is remarkable for the great length and subcylindrical contour of the teeth, the apices of which are slightly recurved. The surface of the tooth is marked by numerous close-set longitudinal grooves, which all terminate at a short distance from the smooth apex. It would appear that the teeth contained large pulp-cavities, and that each was set in a deep and distinct alveolus. Notwithstanding their special peculiarities, these teeth might in many respects be compared with those of the *Teleosauria*.

A metatarsal or metacarpal bone reproduced from a natural cast was shown to be similar to that of a crocodile, but so much shorter in proportion to its thickness as to indicate an altogether shorter and broader foot. The cast of an ungual phalanx, on the other hand, proves that *Stagonolepis* had long and taper claws.

Thus far the resemblances with the *Crocodilia* are, on the whole, very close; but the characters of a coracoid obtained from Lossiemouth separate *Stagonolepis* from all known recent and fossil *Crocodilia*. It is, in fact, a lacertian coracoid, very similar to that of *Hylæosaurus*.

In summing up the evidence thus brought forward as to the affinities of *Stagonolepis*, the author, after comparing it with the oldest known *Reptilia*, expressed his opinion that the peculiar characters of this ancient reptile separate it as widely from the mesozoic *Reptilia* hitherto discovered as these are separated from the cainozoic members of the same group,—in fact, it widely diverges from all known recent and fossil forms, and throws no clear light on the age of the deposit in which it occurs.

The footsteps from the Cummingstone quarries were next described. The largest yet seen by the author are eight or nine inches long, but the majority are much smaller. Prof. Huxley expressed his

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opinion that all the tracks which he had seen were referable to variously-sized individuals of one and the same species of reptile; and he described at length the only perfect impressions he had observed, the one of a fore, the other of a hind foot. The impression of the fore foot presented a broad, oval palmar depression, ending in five digits, of which the innermost, representing the thumb, was very broad and short. Each of the outer digits was terminated by a long and tapering claw; and there were clear traces of a web-like membrane uniting these digits as far forwards as the bases of the ungual phalanges. The innermost digit or thumb is directed inwards as well as forwards, and appears to have been provided with a thick, short, and much curved nail.

The impression of the hind foot is smaller than that of the fore foot, to which, however, it has a general resemblance. It exhibits only four digits, all terminating in taper claws and united by a web. There are indications of a rudimentary outer toe. In one track, where the impression of the fore foot measured three inches, the stride was twelve inches.

The impressions might very well have been made by such an animal as *Stagonolepis*, with the ungual phalanges of which, indeed, the claw-marks of the footsteps present a close resemblance, while the shortness and breadth of the palmar and plantar impressions harmonize very well with the proportions of the metatarsal or metacarpal bone.

In the course of his remarks, the author took occasion to express his great obligations to Mr. Patrick Duff and the Rev. George Gordon for their zealous and most efficient aid, without which it would have been quite impossible for him to lay so complete a case before the Society.

4. "On Fossil Foot-prints in the Old Red Sandstone, at Cummingstone." By S. H. Beckles, Esq. F.G.S.

Mr. Beckles, during a late tour through the Highlands, examined the Sandstone-quarries at Covesea, near Elgin; and, having exposed and removed several square yards of the Sandstone-slabs bearing fossil foot-prints at this place, has sent a large collection of them to London, but has not yet had the opportunity of studying them in detail. Mr. Beckles says that he has secured several varieties of footsteps, differing in size and form, and in the number of the claws, which vary apparently from 2 to 5. One foot-print, of a circular shape, measured 15 inches in breadth. Some of the smaller foot-prints are evidently formed by young individuals of the same species that made some of the larger marks. Some of the prints have been left, in the author's opinion, by web-footed animals.

Most of the surface-planes of the rock, at different levels, bear foot-marks. The majority of the tracks, Mr. Beckles says, are uniserial, the double (or quadrupedal) series being exceptional.

Mr. Beckles noticed also impressions of rain-prints, well-marked on some of the surface-planes, and indicating the direction of the wind blowing at the time of the rain-fall.

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IX. *Intelligence and Miscellaneous Articles.*

THOUGHTS ON THE FORMATION OF THE TAIL OF A COMET. BY J. J. WATERSTON, ESQ.

ANY attempt to account for the formation of the tail of a comet upon exact physical principles would be assisted, if, in the drawings of the telescopic appearances of the nucleus and adjacent coma—which are sometimes provided when circumstances are favourable—we were presented with the projection on the plane of vision of the radius vector, and of the tangent to the orbit. In some cases it might be possible to take such observations as would fix the actual curve of the *exterior line* of the tail, referred to the radius vector, passing through the centre of the nucleus,—*i. e.* supposing the central axis of the tail to be in the plane of the orbit. This line is very marked in the front side of Donati's comet, and might probably be easily fixed by the equatorial.

If we view the tail as composed of molecules as free from the force of cohesion as the molecules of an uncondensable gas, and raised from the nucleus by the heat of the sun, and suppose that that heat as it strikes upon each molecule is converted into a force centrifugal, that not only effectually counteracts the force centripetal of the sun's gravity, but that greatly exceeds it, such molecules will be quickly removed from the feeble attraction of the nucleus, and assume the motion of bodies entirely free from its influence. This hypothesis is suggested by the mechanical theory as a possibility.

If we inquire as to the centrifugal power of the sun's rays, we may, with the data afforded by modern research, easily compute the accelerative force it is capable of engendering on a single chemical molecule, if their whole heating power were converted into such a force. This is surpassingly great—no less than 800 miles per second! The data are, value of sun's radiation in a solar day equal to 1·8 ft. thickness of ice melted; the mechanical equivalent of which is about 13 lbs. raised 1 foot high per second by the heat impinging on a square foot. If this heat impinges on a superficial foot of gold-leaf one molecule thick (about 1–200,000,000th of an inch, as deduced from the relation of *capillarity* to *latent heat*, both being the measure of liquid cohesion, the first that of the superficial stratum of molecules, the second that of a cubic mass of molecules), and is converted into an impulsive force in one direction, it would in one second communicate a velocity of 800 miles a second; and in about four minutes a velocity equal to light itself. It is, of course, an extreme case to suppose the whole heat absorbed and converted; but it seems right to have in view the quantitative elements of the question. While the earth's atmosphere absorbs heat from the sun's rays, we have reason to suppose that heat-vibrations are converted into rectilineal velocity of gaseous molecules; but there is no reason to suppose that the impulse is in the specific direction *from* the sun, but equally *from* and *to*, as in vibratory impulses generally. It may be shown that the mechanical equivalent of the solar heat that im-

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pinges on the earth amounts to one-hundredth of its gravity towards the sun; so that, if any part of it acted centrifugally, the orbit would be disturbed. The same remark applies to a comet; but it cannot be denied that the phænomena of the tail, more especially as it turns sharp round in the perihelion passage, are such as require for their explanation a very active exhibition of such a centrifugal force as might be engendered by the conversion of heat acting upon single and free molecules.—*Proceedings of the Royal Astronomical Society*, vol. xix. p. 29.

ON THE DIFFERENCE PRESENTED BY THE PRISMATIC SPECTRUM OF THE ELECTRIC LIGHT *IN VACUO* AT THE POSITIVE AND NEGATIVE POLES. BY PROFESSOR DOVE.

If I am not mistaken, M. Quet was the first to observe that when as perfect a vacuum as possible is produced in the electric egg, and the wires which enter it are connected with a Ruhmkorff's apparatus, two lights, differing in colour, form, and position, make their appearance upon these wires. One of them is blue, and uniformly envelopes the negative pole; the second, which is of a fiery-red colour, adheres to the positive pole, and stretches thence towards the negative pole, but is separated from the light of the latter by an obscure space. These phænomena may be studied more conveniently by soldering the wires into vacuum-tubes, such as M. Geissler of Bonn prepares with much skill.

If the two lights be observed by absorption in coloured glasses, or by making them illuminate colouring matters, it is seen immediately that we have not to do here with homogeneous light, for both of them may be seen through a cobalt glass of 6 millims. in thickness; all the space which they occupy appears red through a red lens, yellowish through a plate of yellow glass; and they acquire a brownish tint when looked at through a plate of uranium glass which becomes fluorescent under their influence, whilst it appears porcellanous when the electric light at its outer surface is combined by reflexion with the fluorescent light proceeding from its interior. These two lights vanish by the combination of a cobalt glass and a red glass, which only allows the passage of the extreme homogeneous red. It is easy to recognize by their colour several colouring matters when they are illuminated by the positive or negative light.

If the light be allowed to pass through a narrow fissure, and analysed with an equilateral prism of flint glass or sulphuret of carbon, the positive and negative lights furnish different spectra.

A Geissler's tube, of a pear-shape, 7 inches in length, presented the following phænomena:—The spectrum of the blue light at the negative pole showed a large black streak in the blue, and a second similar one at the limits of the blue and green, a very small streak at the limits of the yellow, and nothing in the red. The light of the positive pole gave a continuous violet and blue band, several small streaks in the green, a very black streak at the limits of the yellow, and a small dark streak in the middle of the red. The colours which appear discontinuous in one spectrum are not so in the other.

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A second tube of the same length, of a spherical form in the middle and with conical prolongations at the two ends, presented the same phænomenon at the negative pole; but in the light of the positive pole, independently of the streaks previously indicated, there were two small dark streaks in the blue.

The wires soldered into the glass tubes were of platinum. In the cylindrical electrical egg with brass electrodes, the phænomena are the same as in these tubes, with this difference only, that the negative light between the two broad black bands appears greenish, nearly like the space between F and *b* in the solar spectrum, and that in the spectrum of the positive light, besides the bands in the red and green, several also make their appearance in the blue.

If the point at the negative pole be replaced by a ball, the positive pole remaining the same as before, there appears in the red of the negative light a small dark streak, whilst in the positive light two strong dark streaks become visible in the blue. Thus the phænomenon becomes modified, as if each of the two lights, positive and negative, contained a slight proportion of the other. In every case the spectra remain different, of which it is very easy to convince oneself, when the fissure is long enough to allow the two spectra to be observed at the moment when the prolongation of the one penetrates into the other.

To observe the spectrum of the spark, the points of a spark-micrometer were placed in communication by a thread of glass, and in this way a stream of sparks was obtained, in the light of which the characteristic brilliant streaks were strongly marked.

Non-continuous spectra, such as those above described, are produced by coloured flames, or when a source of white light is submitted to the absorption of coloured gases. Now, as the space which separates the poles is not an absolute vacuum, we may suppose that various gaseous matters, or a single gas in different states, occur at these poles. The two cases suppose, it is true, a certain duration in the action of the pole and an action which increases with this duration. If we introduce a commutator between the pile and the Ruhmkorff's apparatus, the two spectra immediately change, and remain without alteration, which is contrary to the hypothesis in question.

When the luminous current is deflected by means of a strong magnet, no change is observed in the spectra.

The luminous phænomena presented by electricity *in vacuo* exhibit a striking analogy with those of the *aurora borealis*, which has caused them to receive the name of artificial auroræ boreales. The fiery red of the positive pole particularly resembles the red which characterizes many auroras; and the author has of late years repeatedly had the opportunity of remarking this resemblance, especially in two cases of aurora borealis seen at Berlin. The peculiarities presented by the electric light *in vacuo* are so marked, that it appears easy to decide definitively by prismatic analysis whether the light of the aurora borealis is or is not of an electrical nature.—Poggendorff's *Annalen*, 1858, No. 5.

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THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

FEBRUARY 1859.

X. *On the Mean Length of the Paths described by the separate Molecules of Gaseous Bodies on the occurrence of Molecular Motion: together with some other Remarks upon the Mechanical Theory of Heat.* *By* R. CLAUSIUS*.

(1.) THE February Number of Poggendorff's *Annalen* contains a paper by Buijs-Ballot "On the Nature of the Motion which we call Heat and Electricity." Amongst the objections which the author there makes against the views advanced by Joule, Krönig, and myself concerning the molecular motion of gaseous bodies, the following deserves especial consideration. Attention is drawn to the circumstance that, if the molecules moved in straight lines, volumes of gases in contact would necessarily speedily mix with one another,—a result which does not actually take place. To prove that such mixture does not occur, the following facts are adduced (p. 250):—"How then does it happen that tobacco-smoke, in rooms, remains so long extended in immoveable layers?" Mention is also made of the same appearance with clouds of smoke in the open air. Further, "If sulphuretted hydrogen or chlorine be evolved in one corner of a room, entire minutes elapse before they are smelt in another corner, although the particles of gas must have had to traverse the room hundreds of times in a second." Further, "How could carbonic acid gas remain so long in an open vessel?"

These objections may, at first glance, appear to have very great weight; and I consider it therefore necessary to prove, by special considerations, that the facts adduced are perfectly reconcileable with the theory of the rectilineal motion of the molecules. Indeed, I rejoice at the discussion of this point by M. Buijs-

* Translated by Dr. F. Guthrie, from Poggendorff's *Annalen*, No. 10, 1858.

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Ballot, inasmuch as it affords me a desired opportunity of completing this part of my theory (which was perhaps discussed too briefly in my paper), and to prevent thereby further misunderstandings.

(2.) It is assumed in the objections, that the molecules traverse considerable spaces in straight lines; this appears prominently in the second objection, in which it is said that a molecule must have had to traverse the room many times in a second. This assumption can, however, in nowise be considered as a necessary consequence of the views advanced by me concerning the conditions of gases. Amongst the conditions which must be satisfied if Mariotte and Gay-Lussac's law for a gas is true with perfect strictness, I have adduced the following,—"that those portions of the path of a molecule throughout which the molecular forces are of influence in sensibly altering the motion of the molecule, either in direction or velocity, must be of vanishing value compared with those portions of the path throughout which such forces may be considered as inactive." Now in actually existing gases, Mariotte and Gay-Lussac's law is not strictly, but only approximately true; and it hence follows that in them such first portions of the paths of the molecules must be small, but not vanishingly small, compared with the entire paths. Inasmuch, now, as one of the fundamental conditions upon which the whole theory rests is that the molecular forces are only effective at small distances from the molecules, a path which is very great in comparison to the sphere of action of a molecule may yet, considered absolutely, be very small.

By a few simple considerations, an approximate idea may be formed of the mean magnitude of the paths traversed by the separate molecules: I purpose endeavouring to elucidate this in what immediately follows.

(3.) For this purpose it will be advisable to prefix some remarks concerning the manner in which it is possible to view the molecular forces, and what has accordingly to be understood by the sphere of action. These remarks are not to be considered as an essential part of the subsequent development, but are merely intended to fix our ideas.

If we do not take into account the forces of chemical affinity, and only consider such molecules as are chemically indifferent to one another, I imagine that there are still two forces which are to be distinguished. I believe, namely, that when two molecules approach one another an attraction is at first exerted, which begins to be of sensible effect even at some distance, and which increases as the distance diminishes; but that, when the molecules have arrived into the immediate neighbourhood of one another, a force comes into play which seeks to drive them

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asunder. For the view which it is here intended to take, it is indifferent what kind of force this repulsive one is supposed to be, that is, whether, as in the case of solid elastic bodies, it only strives to separate the molecules when they are in actual contact with a force equal to that with which they are pressed together, or whether it is one which begins to act before the actual contact of the molecules. In the same manner, we need not here discuss the question as to the source of these forces, whether they are both to be ascribed to the particles of ponderable matter themselves, or whether one of them is to be referred to a more subtle substance, with which the ponderable particles of matter are furnished.

Let us now imagine two molecules moving in directions such that, if they preserved them unchanged, they would not strike one another, but pass by at some distance. Two cases may here occur. If the distance is very small, the molecules which were drawn towards one another, even from some distance, by the force of attraction, approach so closely that the repulsive force comes into play, and a rebounding of the molecules results. If the distance be somewhat greater, the paths of the molecules only suffer a certain change of direction through the attractive force, without the repulsive force being able to act. Finally, at still greater distances, the effect of the molecules upon one another may be altogether neglected.

How great the distances must be in order that the one or other case might occur, could not be determined universally, even if we possessed exact knowledge of the molecular forces; for the velocity of the molecules and the reciprocal inclination of their paths are of influence. Nevertheless, *mean* values of these distances may be obtained. We will therefore suppose that the distance ρ is given for such a mean value, which forms the boundary between the first and second case, and the meaning of which we will define with greater precision in the following manner:—If the centres of gravity of two molecules have such directions of motion that if they were to proceed in those directions in straight lines they would pass by one another at a distance greater than ρ, then the molecules only change their courses to some extent through reciprocal attraction, without the repulsive force coming into action between them. If, on the other hand, this distance is less than ρ, the latter force also comes into play, and a rebounding of the molecules takes place.

If, now, the latter case alone be considered as one of *impact*, and we do not concern ourselves with the changes of direction which the force of attraction effects at greater distances, we may, for what we have here to consider, represent a sphere of radius

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ρ, described around a molecule and having its centre of gravity for a centre, by the term *sphere of action* of the molecule.

I again call attention to the fact that the special hypotheses here made, concerning the nature of the molecular forces, are not to be viewed as a necessary condition for the developments which follow; their only purpose is to facilitate the comprehension by giving something definite to the imagination. It is of no import how we consider the forces by reason of which the molecules change the directions of their motions; if we but admit that their effects are only sensible at very small distances, we may assume some distance as limiting value for the purpose of being able to neglect the actions from greater distances, and only regard those for smaller ones. A sphere described at this distance may be called a sphere of action.

(4.) If, now, in a given space, we imagine a great number of molecules moving irregularly about amongst one another, and if we select one of them to watch, such a one would ever and anon impinge upon one of the other molecules, and bound off from it. We have now, therefore, to solve the question as to how great is the mean length of the path between two such impacts; or more exactly expressed, *how far on an average can the molecule move, before its centre of gravity comes into the sphere of action of another molecule.*

We will not discuss this question, however, immediately in the form just given: we will propose instead a somewhat simpler one, which is related to the other in such a manner that the solution of the one may be derived from that of the other.

If we assume that not all the molecules present in the space are in motion, but that the one chosen for observation is the only one which moves, and all the rest remain fixed in position, the moving molecule in these circumstances also would strike here and there upon one of the others, and the number of blows which it suffers in this case during one unit of time may be compared with the numbers which it would experience in event of universal movement. On considering the matter more attentively, we are soon convinced that the number of blows amongst moving molecules must be greater than amongst stationary ones, or, which comes to the same thing, that the mean length of the paths which the molecule watched passes over between two consecutive impacts, must be less in the first case than in the second. The relation between the lengths of the two paths may be definitely found as soon as the velocity of the remaining molecules, in comparison with that of the one watched, is known. For our investigations, that case only is of special interest where the velocities of all the molecules are *on an average* equally great. In this case, if we only consider the *mean* velocities, we may

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more simply assume that all molecules move at the same rate; and for this case we obtain the following result:—*The mean lengths of path for the two cases* (1) *where the remaining molecules move with the same velocity as the one watched, and* (2) *where they are at rest, bear the proportion to one another of* $\frac{3}{4}$ *to* 1.

It would not be difficult to prove the correctness of this relation: it is, however, unnecessary for us to devote our time to it; for, in our consideration of the mean path, it is not the question to determine exactly its numerical value, but merely to obtain an approximate notion of its magnitude; and hence the exact knowledge of this relation is not necessary. It is even sufficient for our purpose if we may assume as certain that the mean path among moving molecules *cannot be greater* than among stationary ones; this will certainly be at once admitted. Under this hypothesis, we will confine the discussion of the question to that case *where the molecule watched alone moves, while all the others remain at rest.*

Moreover, without affecting the question in anything, we may suppose a mere moving point in place of the moving molecule; for it is in fact only the centre of gravity of the molecule which has to be considered.

(5.) Suppose, then, there is a space containing a great number of molecules, and that these are not regularly arranged, the only condition being that the density is the same throughout, *i.e.* in equal parts of the space there are the same numbers of molecules. The determination of the density may be performed conveniently for our investigation by knowing how far apart two neighbouring molecules would be separated from one another if the moleeules were arranged cubically, that is, so arranged that the whole space might be supposed divided into a number of equal very small cubic spaces, in whose corners the centres of the molecules were situated. We shall denote this distance, that is, the side of one of these little cubes, by λ, and shall call it *the mean distance of the neighbouring molecules.*

If, now, a point moves through this space in a straight line, let us suppose the space to be divided into parallel layers perpendicular to the motion of the point, and let us determine *how great is the probability that the point will pass freely through a layer of the thickness* x *without encountering the sphere of action of a molecule.*

Let us first take a layer of the thickness 1, and let us denote by the fraction of unity a the probability of the point passing through this layer without meeting with any sphere of action: then the corresponding probability for a thickness 2 is a^2; for if such a layer be supposed divided into two layers of the thickness 1, the probability of the points passing free through the first layer, and thereby arriving at the second, must be multiplied by

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the probability of its passing through the latter one. Similarly, for a layer of the thickness 3, we have a^3, &c., and for a layer of any thickness x we may accordingly write a^x. Let us transform this expression by putting $e^{-\alpha}$ for a, in which e is the base of the natural logarithms, and $-\alpha = \log_e a$, which logarithm must be negative, because a is less than 1. If now we denote the probability of the free passage through a layer of the thickness x by W, we have the equation

$$W = e^{-\alpha x}, \quad \ldots\ldots\ldots \quad (1)$$

and we have only to determine here the constant α.

Again, let us consider a layer of such thinness that the higher powers of the thickness may be neglected in comparison with the first. Calling this thickness δ and the corresponding probability W_δ, the former equation becomes

$$W_\delta = e^{-\alpha\delta} = 1 - \alpha\delta \quad \ldots\ldots\ldots \quad (2)$$

The probability in this case may also be determined from special considerations. Let us direct our attention to any plane in the layer parallel to one of the bounding planes of the layer, and let us suppose all the molecules whose centres lie in the layer to be so moved perpendicular to the layer that their centres all fall upon this plane; we have now only to inquire how great the probability is that the point, in its passage through this plane, meets with no sphere of action; such probability may be simply represented by the proportion of two superficial areas. Of the entire part of the plane which falls within the given space, a certain portion is covered by the great circles of the spheres of action whose centres fall upon it, while the remaining portion is free for the passage; and the probability of the uninterrupted passage is therefore expressed *by the relation of the free portion of the plane to the whole plane.*

From the manner in which the density was determined at the beginning of this article, it follows that in a layer of thickness λ, so many molecules must be contained, that, if they be supposed brought into one and the same plane parallel to the bounding plane, and to be arranged still quadratically in this plane, then the side of the small square in whose corners would be situated the centres of the molecules would be equal to λ. Hence it follows, that the part of the plane which would be covered by the great circles of the spheres of action, would be related to the remainder of the plane as a great circle would to a square of side λ, so that, accordingly, the covered superficial area would be expressed by the fraction

$$\frac{\pi\rho^2}{\lambda^2}$$

of the entire superficial area. In order to ascertain the corre-

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sponding magnitude for a layer of the thickness δ, we have only to multiply the previous fraction by $\frac{\delta}{\lambda}$, that is,

$$\frac{\pi\rho^2}{\lambda^3}\delta;$$

and if this magnitude be subtracted from 1, the difference represents the free portion of the plane as a fraction of the whole plane.

Hence the probability that the point will pass through our plane, or, which comes to the same thing, through a layer of thickness δ, without obstruction, is determined by the equation

$$W_\delta = 1 - \frac{\pi\rho^2}{\lambda^3}\delta; \quad \ldots\ldots (3)$$

and on comparing this expression for W_δ with that given in equation (2), we find that

$$\alpha = \frac{\pi\rho^2}{\lambda^3}, \quad \ldots\ldots\ldots (4)$$

and hence the general equation (1) is transformed into

$$W = e^{-\frac{\pi\rho^2}{\lambda^3}x}. \quad \ldots\ldots\ldots (5)$$

(6.) By means of this equation we can now determine the mean value of the path which the point has to traverse before it meets with a sphere of action.

Let us suppose that a great number (N) of points are thrown through space in one direction, and let us suppose the space to be divided into very thin layers perpendicular to the direction of motion; then a small number of the points would be detained in the first layer by the spheres of action, another lot in the second, another in the third, and so on. If, now, each of these small numbers be multiplied by the length of path, the products added, and the sum obtained divided by the whole number N, the quotient will be the mean length of the path which we seek.

According to equation (5), the number of points which either reach or pass the distance x from the commencement of the motion is represented by

$$Ne^{-\frac{\pi\rho^2}{\lambda^3}x};$$

and accordingly the number which reach or pass the distance $x+dx$ is expressed by

$$Ne^{-\frac{\pi\rho^2}{\lambda^3}(x+dx)} = Ne^{-\frac{\pi\rho^2}{\lambda^3}x}\left(1 - \frac{\pi\rho^2}{\lambda^3}dx\right).$$

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The difference of these two expressions, namely,

$$N e^{-\frac{\pi\rho^2}{\lambda^3}x} \cdot \frac{\pi\rho^2}{\lambda^3} dx,$$

represents the number of those points which are detained between x and $x+dx$. The path traversed by these points may be considered as x if we neglect infinitely small differences; and hence the above expression must be multiplied by this length in order to obtain one of the products mentioned before, namely,

$$N e^{-\frac{\pi\rho^2}{\lambda^3}x} \cdot \frac{\pi\rho^2}{\lambda^3} x dx.$$

If, now, it be desired to obtain the sum of all products of this kind which correspond to the several layers of the thickness dx, this must of course, in the case in point where the layers are infinitely thin, be effected by integration. Hence the above formula has to be integrated from $x=0$ to $x=\infty$, whence the following expression is obtained,

$$N \frac{\lambda^3}{\pi\rho^2}.$$

This expression has now only to be divided by N in order to arrive at the mean length of path required. If this be called l', the equation is

$$l' = \frac{\lambda^3}{\pi\rho^2}. \quad \ldots\ldots\ldots \quad (6)$$

In the case where not one molecule only is in motion while all the others are at rest, but where all molecules move with equal velocity, the mean length of way, as mentioned before, is less than that above considered in the proportion of $\frac{3}{4}$ to 1. Hence if we put the simple letter l for this case, we have

$$l = \frac{3}{4}\frac{\lambda^3}{\pi\rho^2}. \quad \ldots\ldots\ldots \quad (7)$$

Writing this equation in the form

$$\frac{l}{\rho} = \frac{\lambda^3}{\frac{4}{3}\pi\rho^3}, \quad \ldots\ldots\ldots \quad (7a)$$

a simple law results. It follows from the manner in which we determined the density, that the part of the given space filled by the spheres of action of the molecules is related to the whole given space as a sphere of action to a cube of the side λ, that is, as

$$\frac{4}{3}\pi\rho^3 : \lambda^3.$$

Accordingly the meaning of the previous equation may be so put:—*The mean length of path of a molecule is in the same pro*

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portion to the radius of the sphere of action as the entire space occupied by the gas, to that portion of the space which is actually filled up by the spheres of action of the molecules.

(7.) In order to have a definite numerical example, let us assume, in round numbers, that the spheres of action of the molecules are so small that only a thousandth of the space occupied by the gas is really filled out by the spheres of action, and that the whole remaining space be free for motion.

For this case we have

$$\frac{\lambda^3}{\frac{4}{3}\pi\rho^3} = 1000,$$

whence it follows that

$$\frac{\lambda}{\rho} = 16{\cdot}12. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

On applying these values we obtain from equations (6) and (7),

$$l' = 1333\rho = 83\lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$l = 1000\rho = 62\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The first expressions in both equations show that, under the assumption made, the mean path has a considerable length in comparison to the radius of the spheres of action, and that therefore, as far as the effect of this circumstance is concerned, Mariotte and Gay-Lussac's law may be very nearly true for the gas. By a simple calculation it may be shown that the relation of 1000 to 1 completely suffices, even for those approximations found by Regnault with permanent gases. It follows that the magnitude of the spheres of action which was taken for illustration, although arbitrarily chosen, may yet be regarded as one within the bounds of possibility.

But if we now regard this same mean value of the length of path in such a manner as to compare it, not with the sizes of molecules, but with our usual units of length, we obtain totally different relations. In all physical and chemical investigations in which opportunity presents itself for drawing conclusions concerning the weight and size of the separate molecules, we are invariably led to the conclusion that, compared with all measurable magnitudes, molecules must be of extraordinarily small size. As yet, no one has been able to establish a bounding line on the other side (for smallness). Accordingly, when an ordinary unit of measure, *e. g.* a litre, is filled with gas at the ordinary atmospheric pressure, we must assume that the number of molecules present is very great, and that consequently the distances between the molecules is very small. Accordingly the values previously found for l' and l, namely, 83λ and 62λ, must only be regarded as small magnitudes.

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(8.) After the above determination of the length of the *mean* path, we still have to consider how the separate paths which really occur are related to the mean path.

The first question which presents itself is, in what proportion is the number of cases in which the real path is less than the mean path, to that of the cases in which it is greater. For answering this question, use is made of (5), in which we have only to substitute the mean value l' for x in order to find what probability there is that the true path is equal to or greater than the mean one. If for l' we here make use of the expression in (6), and denote the corresponding value of W by W_1, then

$$W_1 = e^{-1} = 0{\cdot}3679. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

From the above equation it follows, that out of N cases only 0·3679 N occur in which the real path is equal to, or greater than the mean one, while in the

$$. \quad . \quad . \quad 0{\cdot}6321\ N$$

cases the true path is the smaller one.

If, further, it be required to know the number of cases in which the true path is equal to or above the double, treble, &c. of the mean one, the same process may be adopted as before. Calling the probabilities in question W_2, W_3, &c., we have

$$\left.\begin{aligned} W_2 &= e^{-2} \\ W_3 &= e^{-} \\ &\text{\&c.} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

These numbers evidently diminish very rapidly, since, for instance, $e^{-10} = 0{\cdot}000045$; and we gather from this that, although in isolated cases a molecule may traverse a path considerably longer than the mean one, such cases are comparatively rare, and that in the majority of cases the actual path is smaller or very little larger than the small mean value found above.

(9.) If, now, these results be applied to the externally recognizable behaviours of a gas, in which it is presumed that no other motion common to the whole mass besides the molecular one is present, it is easy to convince oneself that the theory which explains the expansive force of gases does not lead to the conclusion that two quantities of gas bounding one another must mix with one another quickly and violently, but that only a comparatively small number of atoms can arrive quickly at a great distance, while the chief quantities only gradually mix at the surface of their contact.

From this it is clear why clouds of smoke only slowly lose their form on quiet days. Even when the air is in motion, provided such motion consists of a uniform one of the entire cur-

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rent, a cloud of smoke may be carried off without quickly losing its form. Both the other facts adduced by Buijs-Ballot also admit of simple explanation. The remark made by him attached to one of his objections, that the molecules of gas in a room must traverse the room many hundred times in one second, is completely foreign to the theory. Perhaps it might be said of a remark which occurs in the mathematical development attached to my previous paper, that it afforded reason for such an idea. I assumed there, namely, that the gas was in a very flat vessel, and I then assumed that the molecules of gas without disturbing one another, sped backwards and forwards between the two great parallel sides. Nevertheless, this assumption was there introduced with the following words: "In estimating the pressure, *instead of regarding the motion as it really occurs*, we may introduce certain simplifications." I believe I thereby called sufficient attention to the fact that this assumption should not serve to furnish an image of the real process, but only to simplify the calculation there intended, the result of which could not be thereby changed.

XI. *Remarks on Ice and Glaciers.*

By JOHN TYNDALL, *F.R.S. &c.**

THERE are two or three points connected with ice and glaciers to which I intended to refer in a paper now ready to be presented to the Royal Society, but which, on reflection, I think may be more fitly treated in the pages of the Philosophical Magazine.

In the December Number of that Journal there is a reprint of a paper by Prof. W. Thomson, which first appeared in the Proceedings of the Royal Society: the following points in this paper need clearing up.

In the last paragraph Professor Thomson expresses the difficulty which he experiences in accounting for the resolution of a mass of ice into six-petaled liquid flowers†. That such a difficulty should present itself is, I acknowledge, a matter of some surprise to me. The effect, as stated in my paper, is manifestly due to the crystallization of the substance. Water, when crystallizing, builds itself up into flowers of this kind. Last August I observed a magnificent example of this six-rayed architecture upon the summit of Monte Rosa; Dr. Scoresby has given us numerous drawings of polar snow-crystals illustrating the same point; and the drawings of Mr. Glaisher also illustrate it. Not only do snow-crystals exhibit this structure, but *in freezing water*

* Communicated by the Author.

† Phil. Trans. part 1, 1858, pp. 212–213. Phil. Mag. Nov. 1858, p. 334.

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star-shaped masses are formed exactly similar to the liquid spaces produced by the sunbeam in my experiments. The process of liquefaction is simply an inversion of the process of solidification; and to me it seems perfectly natural that the phænomena should be of this complementary character.

Prof. Thomson also objects to my conclusion, that it might be inferred from the experiments on internal liquefaction that ice was a uniaxal crystal*. I look at the matter purely from a physical point of view; and thus regarded, nothing, I think, can be more certain than the conclusion referred to. A line may be drawn through a mass of ice, round which the particles are arranged with perfect symmetry; and there is but *one* direction through the substance round which this symmetry prevails. To me it appears certain that this line will be the optic axis of the crystal, and that the crystal will possess but one such axis. The liquefaction experiments prove the facts; and all that we know of optical phænomena confirms the conclusion.

In the last paragraph but one, Prof. Thomson writes as follows:—"I believe the theory I have given above contains the explanation of one remarkable fact observed by Dr. Tyndall in connexion with the beautiful set of phænomena which he discovered to be produced by radiant heat, ... the fact namely that the planes in which the vesicles extend *are generally parallel to the sides* when the mass of ice is a flat slab; for the solid will yield to the 'negative' internal pressure due to the contractility of the melting ice, most easily in the direction perpendicular to the sides. ..... Hence the vesicles of melted ice, or of vapour caused by the contraction of the melted ice, must, as I have shown, tend to place themselves parallel to the sides of the slab."

Now the fact is, that the melting of the ice is totally independent of the sides of the slab. If the sides do not coincide with the surfaces of freezing, the planes of the flowers will *not* be parallel to the sides. The effect is not due to any mechanical weakness dependent on the form of the mass operated on; but the interior melting is solely dependent on the interior crystallization. No matter how irregular in form the fragment of ice may be, the sending of a calorific beam through it reveals at once its planes of freezing; for the flowers always form parallel to these planes, no matter what the direction of the beam through the mass may be. This I have distinctly stated. I may add, that in the single case recorded in my paper to which Mr. James Thomson's theory is applicable, I have applied that theory myself.

Prof. J. D. Forbes has recently described before the Royal Society of Edinburgh some experiments that he has made on

* Phil. Trans. part 1, 1858. Phil. Mag. Nov. 1858, p. 336.

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the freezing together of masses of ice with moist surfaces. His explanation of the effect is briefly this:—When one surface is "brought up nearly to physical contact" with the other, "there is a double film of viscid water isolated between two ice surfaces colder than itself. . . . . Part of the sensible heat which it (the film) possesses is given to the neighbouring strata which have less heat than itself, and the intercepted film of water in the transition state becomes more or less perfect ice."

He finds, in fact, by experiment, that "masses of strong ice which had already for a long time been floating in unfrozen water-casks, or kept for days in a thawing state, being *rapidly pounded*, showed a temperature of 0°·3 F. below the true freezing-point." And "water being carefully frozen into a cylinder several inches long, with the bulb of a thermometer in its axis, and the cylinder being then gradually thawed, or allowed to lie for a considerable time in pounded ice at a thawing temperature, showed also a temperature decidedly inferior to 32°, not less than 0°·35 F."

The cause here suggested, namely that the moist film is frozen by the cold which the ice possesses, is that which first occurs in seeking an explanation of the phænomenon in question; and it was with reference to it that I instituted the long series of experiments on artificial ice, recorded in my paper "On some Physical Properties of Ice," and which lead to conclusions opposed to those of Prof. Forbes.

Why is the "rapid pounding" necessary in the experiment of Prof. Forbes? Doubtless in order that the ice may be brought into contact with the thermometer *before* its temperature has risen to 32°. But give the ice time to rise to 32°; let its last residue of cold be abolished—the mass thus warmed, and in which the finest thermometer will not show the smallest fraction of a degree below 32°, may, with the utmost facility, be converted by pressure into *solid ice*.

Let the thawing surface of a mass of ice be scraped away, so as to obtain a fine ice-powder possessing the temperature of that surface. Let not the alleged magazine of cold *within the ice* be at all called upon; such a powder, or more properly fine slush, the temperature of which no thermometer can show to be below 32°, may, as in the former case, be converted by pressure into solid slabs of ice.

Further, much of the ice which I made use of in my experiments was full of cells partially filled with liquid water: the ice containing them could not be lower than 32°, and was never observed to be lower. Water, it is true, will remain liquid below 32°; but normal ice, at the atmospheric pressure, cannot be melted by a temperature below 32°; and I have shown that such

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ice, in contact with an internal cavity, may be liquefied by the heat which has been conducted through the substance. The water thus produced must have a temperature of at least 32°; but *the ice has been the conductor of this temperature,* and therefore cannot possibly be under 32°. Such ice, however, exhibits all the phænomena of regelation. Slabs of it freeze together; and if crushed and pressed, it can be welded and moulded in the manner which I have more than once had occasion to describe.

These facts, I think, prove that the explanation of Prof. Forbes does not apply to the phænomena. I agree with him that "contact without pressure" produces regelation, and, I think, for reasons which have been already assigned.

There is one more point on which, in justice to myself and to others, I feel called upon to say a few words.

Since the publication of the paper by Mr. Huxley and myself*, I have been reminded by more than one writer, that Prof. Forbes had himself abandoned the theory of the veined structure which was examined, and, I think, proved to be untenable, in the paper referred to. Prof. Thomson, for example, states that, in his thirteenth letter upon glaciers, Prof. Forbes "formally abandons the notion that the blue veins are due to the freezing of infiltrated water†."

The second section only—a very short one—of the thirteenth letter refers to this subject. The title of that section is simply "On the Conversion of Névé into Ice;" and there is not a word referring to a new theory of the veined structure. In the first paragraph Prof. Forbes formally mentions the subjects of which he is going to treat: he says, "I shall now add a few observations tending to throw light on two of the most obscure glacial phænomena: first, the conversion of the snow of the névé into pure ice; and secondly, on the apparent ejection of stones from the surface of the glacier:" there is not a syllable about abandoning a theory which formed the most important part of his glacier investigations.

Before the publication of the paper by Mr. Huxley and myself, every fact or speculation of any importance, in the work of Prof. Forbes, was vividly present to my mind. With regard to the conversion of névé into ice, I knew that he had expressed himself thus:—"No doubt the transition is effected in this way:—the summer's thaw percolates the snow to a great depth with water; the frost of the succeeding year penetrates far enough to freeze

* Phil. Trans. part 2, 1857. Phil. Mag. 1858, vol xv. p. 365.
† Phil. Mag. S. 4. vol. xvi. p. 465.

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it, at least to the thickness of one year's fall, or by being repeated in two or more years consolidates it more effectually. Thus M. Elie de Beaumont most ingeniously accounts for the alleged non-existence of glaciers between the tropics by the fact that the seasons have no considerable variations of temperature, and the thaw and frost do not separately penetrate far enough to convert the snow into ice." (Travels, p. 31.)

This passage naturally rose to my mind when I read the following in the thirteenth letter upon glaciers:—"I am satisfied, then (and it is only after long doubt that I venture this confident expression), that the conversion of snow into ice *is due to the effects of pressure* upon the loose and porous structure of the former." This is the only formal retractation which the letter contains; and I think I was justified in regarding it as an abandonment of the view expressed in the passage first quoted, and not as an abandonment of the theory of the veined structure.

The only mention of "infiltrated water" which the section referred to by Prof. Thomson contains, is in its last paragraph, where it thus occurs:—"We are therefore relieved from the difficulty of accounting for the cold which would be necessary to freeze the infiltrated water which was [I] at one time believed necessary TO EXPLAIN THE CONVERSION OF THE NÉVÉ INTO PROPER ICE:"—not a word about the veined structure.

In no part of this letter, that I can find, does Prof. Forbes state that he has abandoned his first theory of the structure; and one sentence alone could, by implication, be construed into such an abandonment. Referring to a particular observation made on the Talèfre glacier, he concludes "*that the conversion into ice is simultaneous and, in this case, identical with the formation of the blue bands.*" I am quite willing to accept whatever interpretation Prof. Forbes chooses to attach to this sentence; but in justification of myself I would ask, is it likely that a theory of such importance, and on which so much labour had been expended, was meant to be broken down and rebuilt by a sentence of this kind? I would here remark that this thirteenth letter can only be appreciated by those who have made themselves perfectly conversant with what Prof. Forbes had before written. In his book, in his previous letters, in his papers in the Philosophical Transactions, in his controversial discussions, the same view is constantly advocated.—The blue veins are "undoubtedly infiltrated crevices," the crevices being produced by "differential motion." "Mr. Hopkins," writes Prof. Forbes in 1845, in reference to an experimental proof, "denies that the ribboned (veined) structure is produced by differential motion, ... *No person who has seen the model made, or even been told how it was made, and inspects the ribboned structure upon its sur-*

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face, can, I think, unless influenced by previous theoretical views, entertain any other opinion." Is it to be supposed that convictions thus strongly uttered, based upon years of observation, and established, according to the above quotation, by the testimony of the senses themselves, are meant to be reversed by a single observation which, after all, is essentially defective, involving, in reality, not a *fact*, but an *opinion*? The supposition is unreasonable, and will appear still more so when it is remembered that throughout the entire letter Prof. Forbes never once tells us that he has changed his views regarding the origin of the veined structure, though he *does acknowledge a change of view upon a different subject.*

On the conversion of névé into ice the latter is sufficiently clear; on the subject of the structure it is altogether vague and unsatisfactory. Prof. Forbes refers to pressure, as he did in his earliest communications upon this subject; but by far the most reasonable interpretation here is, that he regarded the pressure as influential in producing the "differential motion," which, he distinctly states, "*necessarily takes place under intense pressure.*" This interpretation is supported by the fact that we have "lines of tearing" and "incipient fissures" invoked, as formerly, in this letter. "The imprisoned air," writes Prof. Forbes, "is distributed *in the lines of tearing*, in the form of regular globules, just as in the case of the banded lavas which have been so well described by Mr. Darwin." The words "in the lines of tearing" are put in italics by Prof. Forbes himself. Now it is very remarkable that, in the passage of Mr. Darwin's work to which reference is here made, Prof. Forbes's *first theory* of the structure is referred to, *and assumed to be correct.* In his reciprocal reference to Mr. Darwin's theory, Prof. Forbes endorses the comparison of that eminent naturalist, and *does not use a word which would lead us to suppose that he wished to modify Mr. Darwin's assumption.*

With regard to the origin of the veined structure, the letter, as already stated, is so vague that it is impossible to infer from it, with any certainty, what the views of Prof. Forbes on the subject really are. But those who, like myself, have taken the trouble to acquaint themselves with the labour and the learning expended in establishing the first theory, would, I am satisfied, be the last to suppose that it was intended, in the letter referred to, to dispose of that theory in so indirect and summary a way.

Royal Institution, January 1859.

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XII. *On Deep-sea Explorations.* By Professor W. P. Trowbridge*.

OUR present knowledge of the depth of the sea in all quarters of the globe may be compared to the ideas which existed in the minds of men with regard to the form of the continent of America, after the first voyages of the old Spanish and English navigators. Previous to the discovery of Columbus, the most exaggerated notions were entertained of the boundless extent, the unfathomable depths, and the dark wastes of that great chaos of waters which no man had yet dared to explore.

The discovery of the great navigator was quickly followed by the explorations of Ponce de Leon, Cabot, and others, who touched upon various points of the unknown land, and bore back to the old world trophies from the new. Many of the charts made by these bold mariners are still preserved as memorials of their achievements; but what a contrast do they present to the maps of the present day! in most of them scarcely a resemblance can be traced to the form of the continent which we now inhabit. The world does not cease to honour these adventurous pilots because their first efforts were not entirely successful: subsequent researches, with the help of continued improvements in the art of navigation, and in astronomical science, corrected their errors without detracting from their merits. But while these vague notions with regard to the superficial extent of the sea have been removed, and its surface measured with all desirable accuracy, the veil of mystery still obscures its depths. The bottom has been reached at various points; and the world has just witnessed the wonderful stride in human progress to which the first movements in this new field of scientific investigation have led. It is therefore legitimate now to review what has been done,—not with a view of criticising the works of those who have been foremost in these discoveries, but with the better motive of seeking for truth, and stimulating to renewed efforts those who have already done so much, by suggesting probable causes of error in the results which have been obtained, so that new methods may be devised, if necessary, for establishing, with the certainty which science and the popular mind now require, the true form of that portion of the solid crust of the earth which lies concealed beneath the waters of the ocean.

The question of the character of the thin covering of the bottom has undoubtedly been settled by the examination of specimens brought to the surface. And here we must digress somewhat, to refer to the labours of those who were first instrumental in inaugurating deep-sea explorations.

* From Silliman's American Journal for November 1858.

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In the year 1845, Lieutenant (now Commander) Charles H. Davis, U.S. Navy, while running a line of deep-sea soundings across the Gulf-stream, under the direction of the Superintendent of the Coast Survey, obtained one cast of 1350 fathoms, and brought up a specimen of the bottom with the "specimen-cup" of Lieut. H. S. Stellwagen, U.S. Navy. With regard to this sounding, Lieut. Davis remarked as follows:—"U.S. Brig 'Washington,' Oct. 29th, 1845. Sounded with 1300 fathoms line (1¼ mile), and found bottom at that depth. . . . After the lead was felt to strike the bottom, the line became slack, so that the quartermaster could haul it in hand over hand. . . . It appeared to fall off again from the side of the bank, and took the remainder of the line, amounting to 1350 fathoms" (in all). "*The cup came up filled with a greenish mud, which is preserved.* . . . At 3 P.M. sounded again with 715 fathoms line, and found the same bottom as before." The cup referred to was the Stellwagen cup, which is used by some at the present day. In the explorations of Lieut. Davis, 95 specimens of the bottom, and 25 specimens of water at various depths, were brought up and preserved*.

* See Reports of Superintendent of Coast Survey for 1845, 1846, 1847, 1848.

The following interesting observations are cited from the Report for 1847, p. 25:—"Collections of specimens of the bottom from soundings in this section were first commenced by the late Lieutenant Commanding, George M. Bache, U.S. Navy, and have been added-to every year since 1844, placed in small bottles for easy inspection, and duly labelled. It was the intention of that lamented officer to have classified them during the past winter, and to have placed characteristic specimens upon an offshore chart on a suitable scale, thus enabling the eye to generalize the results so as to reproduce them methodically arranged, as upon a geological map. In addition to this, the microscopic examination of these specimens could hardly fail to develope interesting facts in regard to them, some of which might prove of importance to navigators, as all would be to general science. Professor J. W. Bailey of West Point, kindly commenced an examination of this sort. 'All the deep-sea soundings,' he says (in a letter to me on the subject), 'are of the highest interest, being filled with organisms, particularly the calcareous Polythalamia, to an amount that is really amazing—hundreds of millions existing in every cubic inch of these green muds.

"'The most interesting specimen is the one labelled No. 1, latitude 38° 04′ 40″, longitude 73° 56′ 47″, 90 fathoms. This is crowded with Polythalamian forms, *mostly large enough to be recognized by a practised eye without the aid of a magnifier.*' A figure or specimen which would enable a practised eye to recognize these forms—and Professor Bailey has promised to illustrate these researches by drawings—would render even this first inspection of practical value. The 'large Textularia, having the orm of a *truncated pyramid*, which characterizes No. 8, latitude 39° 31′, longitude 72° 11′ 20″, 89 fathoms, not being mistakeable for the forms abundant in No. 1,' it is to be hoped that Professor Bailey will carry so promising a subject to a conclusion. He may add another to the aids furnished by science to navigation."

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In the succeeding year, in the same explorations, soundings were made to the depth of 1500 and 2160 fathoms, without finding bottom: but in the latter case the temperature of the water was recorded at the depth named. In 1848, in the explorations off Cape Hatteras, the officer engaged in the explorations lost his instrument, with 3300 fathoms of line out.

These Gulf-stream explorations were undoubtedly the first systematic deep-sea explorations ever undertaken.

Our principal object, however, is to notice those great depths where *no bottom was found*, and to examine whether the failure *to find the bottom* was, under the circumstances, any proof that it did not exist at much less depths than those reported, or whether any conclusion whatever can be derived from the results.

When we reflect that two-thirds of the earth's surface is covered with water, while the remaining third is dry land, and that the figure of the solid part can only be known when we can trace with certainty the mountain-ranges and valleys along the bottom of the sea, it becomes important to scrutinize those reported measurements which give such enormous depressions in different parts of the sea, compared with which the highest mountain-ranges are insignificant elevations. Numerous instances have been reported in which soundings have been made to the depth of five, six, seven, eight, and nine miles without finding bottom; and again over large areas the bottom of the sea is represented as a comparatively level plain submerged to the depth of two, three, or four miles. Supposing these reported measurements to have been correct, we should have still very insufficient data for arriving at any correct conclusions with regard to the elevations and depressions of the ocean-bed. What idea could be formed, for instance, of the topography of our country, if our knowledge of its surface consisted in knowing the height, above the level of the sea, of only one point in every State of the Union. Such points, selected at random, might be the highest or lowest points within an area of some thousands of square miles; and after all, we should only know that it was possible to measure those heights, without being able to conjecture even their relation to each other. In the case of deep-sea soundings, we only know that bottom has been reached,—in some instances at depths which show that our ideas concerning the unfathomable abysses of the ocean have been erroneous, and to sustain the belief that the mean depth is less than has been supposed. With regard to the uncertainties of the measurements, it is not sufficient to say that, compared with the immense area over which they are spread, the depths are very small; it might as well be argued that the height of the Alps is insignificant compared with the distance around the earth, and therefore an

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error in height of one or two miles is unimportant, or that the elevations of ordinary mountain-ranges need not be noticed when compared with the area of a continent. We are dealing with finite quantities, not with the infinite with which they may be compared; and an error of several thousand feet in two or three miles is hardly within the limits of scientific accuracy.

Prominent among the instances of these reported unfathomable depths, stands the sounding of Captain Denham of the British Navy, in H.M.S. 'Herald,' made in October 1852, on a voyage from Rio de Janeiro to the Cape of Good Hope. This is an extreme case; but since it is reported among the greatest deep-sea casts, it will serve best for illustration*. *All other great casts of the lead which have been reported are subject to the same causes of error*, which are to be found in this, some in a greater and some in a less degree; so that it is not necessary for us to believe yet anything with regard to them, except that they gave no result. The sounding of Captain Denham was made with a lead weighing nine pounds, attached to a line one-tenth of an inch in diameter; and it is reported that this lead descended to the depth of nearly nine miles in the sea without touching bottom.

In accordance with a plan which originated with the lamented G. M. Bache, United States Navy, in 1846, in the explorations of the Gulf-stream, and which has constantly been followed since, Captain Denham noted the time of running out of the successive portions of the sounding-line during the nine hours of its supposed descent. According to these observed times of descent, the nine-pound lead communicated to the descending line at the depth of 3000 fathoms, or 18,000 feet, a velocity of *two feet* per second; a result which is *philosophically impossible*, since the resistance of the water acting upon a line of this diameter, moving with a velocity of two feet per second, at the depth mentioned, amounts to more than three times the weight of the lead or shot used. It will hardly be necessary to enter into any argument to show that there can be no motion of descent when the resistance to that motion is three times the weight of the moving mass. Further, the observations show that the nine-pound shot and line were running with a velocity of two feet and a half per second at the depth of 2000 fathoms, or 12,000 feet. Here the result contradicts in quite as strong a manner the mechanical laws of the descent; and in fact below 1000 fathoms, or 6000 feet, if we credit the observations, a velocity was observed in the running out of the line which it was impossible for the lead to communicate to it. In fact but a small part of that velocity could have

* Lieut. Maury discusses these deep casts in his sailing directions; but his rules for arriving at the depth do not seem to me to be entirely satisfactory.

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been produced by the descent of the lead. Here we have a reliable result to the depth of 1000 fathoms only. The difference between this result and the conclusions of Captain Denham is simply the difference between *one mile* and *nine miles*.

In measuring the distance to the sun, an error of eight miles would hardly be worth noticing, perhaps; but what conclusions can be drawn from a measurement in which the probable error amounts to eight times the whole distance?

Popular ideas with regard to the sinking of bodies in the sea have heretofore been vague,—for the reason, perhaps, that the laws which govern this descent, and which are derived from the well-known laws of fluids, have never been fully defined in their application to the depths of the ocean. Some imagine that ships which founder at sea, sink to a certain depth, and there float about until broken to pieces, or thrown upon some bank beneath the sea; and, indeed, a recent writer in England has published a book sustaining this absurd notion. Others, again, believe that the buoyant force of the water at great depths is enormous, and due to the whole pressure of the column of water above, and that all bodies which are lighter than water at the surface, will, if sunk to the bottom and detached from the sinker, shoot upward with a great velocity; or, in other words, that the *density* of the water increases directly with the depth. These views are erroneous. It is true the pressure increases with the depth, to the amount of fifteen pounds upon every square inch for every thirty-four feet in depth; but the density is not thereby sensibly increased, owing to the incompressibility of the water; so that neither the buoyant force, nor the resistance to the motion of any body, are sensibly increased from the surface to the bottom. At the depth of 3000 fathoms, for instance, the pressure upon a square inch is nearly 8000 pounds, but the column of 18,000 feet of water is only shortened about 60 feet: the density is thus but slightly increased; but the effect of this enormous pressure upon compressible bodies, as air, wood, &c., is to condense them into a smaller bulk, by which they may be rendered *heavier than water*, and will sink of their own weight. A piece of wood cannot float at the bottom of the sea; but a very slight extraneous force will bring it to the surface.

Now, how is it with the sounding-lead and line? The lead, if allowed to descend alone, will fall with a uniform and rapid velocity to the bottom. This velocity will be attained within a few feet of the surface, and will be due to the opposing forces of gravity and the resistance of the water, which will be balanced when the uniform velocity is reached. But if a line be attached to the lead, a few hundred feet of the line will offer a resistance to the motion nearly equal to the whole weight of the lead; and

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as successive lengths of line are drawn into the water, the resistance is constantly increased; so that at 2000 or 3000 fathoms depth, the weight will be almost entirely suspended in the sea by the resistance of the water along the sides of the line.

Some idea of the resistance which opposes the motion of a sounding line may be formed from the fact, that upon 1000 fathoms of a line one-tenth of an inch in diameter, moving with a velocity of three feet per second, the resistance is between twenty-five and thirty pounds. And if the velocity be increased to six feet per second, the resistance upon the line becomes a hundred pounds nearly. Or, if the length of the line be doubled, with the same velocity, the resistance is doubled; and it is also directly proportional to the diameter of the line.

These are some of the reasons why an improvement in the mode of measuring the depths of the sea is not only desirable, but necessary, before a certain knowledge of those depths can be obtained.

XIII. *On the Direction of the Vibrations of a Polarized Ray.* *By* Professor Challis*.

THE existing state of the question as to the direction of the vibrations of a polarized ray, so far as that question has been considered on the principles of the theory of light which ascribes the phænomena to the oscillations of the discrete atoms of the luminiferous medium, cannot perhaps be better stated than by quoting the following passage from Dr. Lloyd's 'Address' at the Meeting of the British Association at Dublin in 1857:—

"The vibrations of a polarized ray are all parallel to a fixed direction in the plane of the wave; but that direction may be either *parallel* or *perpendicular* to the plane of polarization. In the original theory of Fresnel the latter was assumed to be the fact; and in this assumption Fresnel has been followed by Cauchy. In the modified theories of MacCullagh and Neumann, on the other hand, the vibrations are supposed to be parallel to the plane of polarization. This opposition of the two theories was compensated, as respects the results, by other differences in their hypothetical principles; and both of them have led to conclusions which observation has verified. There seemed, therefore, to be no means left to the theorist to decide between these conflicting hypotheses, until Professor Stokes, recently, in applying the dynamical theory of light to other classes of phænomena, found one in which the effects should differ on the two assumptions. When light is transmitted through a fine grating, it is

* Communicated by the Author.

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turned aside, or *diffracted*, according to laws which the wave-theory has explained. Now Professor Stokes has shown that, when the incident light is *polarized*, the *plane of vibration* of the diffracted ray must differ from that of the incident, the two planes being connected by a very simple relation. It only remained, therefore, for observation to determine whether the *planes of polarization* of the incident and refracted rays were similarly related, or not. The experiment was undertaken by Professor Stokes himself, and he has inferred from it that the original hypothesis of Fresnel is the true one; but, as an opposite result has been obtained by M. Holtzmann on repeating the experiment, the question must be regarded as still undetermined. The difference in the experimental results is ascribed by Professor Stokes to the difference in the nature of the gratings employed, the substance of the diffracting body being supposed to exert an effect upon the polarization of light, which is diffracted by it under a great obliquity. I learn from Professor Stokes that he proposes to resume the experimental inquiry, and to test this supposition by employing gratings of various substances. If the conjecture should prove to be well founded, it will, unfortunately, greatly complicate the dynamical theory of light."

It must be admitted that the question thus presents a very unsatisfactory aspect. Considering the amount of talent that has been brought to bear upon it, and the great length of time during which it has been under discussion, I feel confirmed by the above statements in the opinion I have long entertained, that there is no foundation for the *oscillatory* theory of light. The having recourse to a *diffraction* experiment to settle a point which it is scarcely possible that a good theory applied to any instance of *polarization* could leave undecided, appears to be only a symptom of a failing theory. It must be borne in mind that in this theory the constitution of the luminiferous medium is entirely hypothetical, and that the hypotheses were framed expressly to account for transverse vibrations, which were found by experiment to be necessary to explain certain phænomena of light. It is not likely, I think, that this course would have been taken, if it had been known in Fresnel's time that the free vibrations of a medium constituted like air, are both longitudinal and transversal, as I have shown to be the case on the principles of hydrodynamics. Since, therefore, the hypothetical medium of the oscillatory theory has hitherto failed to determine in what direction the vibrations of a polarized ray take place, it seems to be the proper time to inquire what answer the question receives from the undulatory theory of light treated according to hydrodynamical principles. This is what I now propose to do.

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In the mathematical theory of vibrations given in two papers contained in vol. viii. part 3 of the Cambridge Philosophical Transactions, and in a communication to the Philosophical Magazine for December 1852, I have shown that the free vibrations of an aëriform medium in their simplest form, consist of longitudinal vibrations parallel to an axis, and transverse vibrations which at each instant are the same in a given transverse plane at all equal distances from the axis. The longitudinal velocity V, the transverse velocity w, and the condensation σ, at any point whose coordinates along and perpendicular to the axis are z and r at the time t, are expressed by the equations,

$$V=f\frac{d\phi}{dz},\quad w=\phi\frac{df}{dr},\quad \kappa^2a^2\sigma=-f\frac{d\phi}{dt},$$

$$\phi=-\frac{m\lambda}{2\pi}\cos\frac{2\pi}{\lambda}(z-\kappa at+c),$$

$$f=1-\frac{(2r)^2}{\lambda^2}+\frac{(2r)^4}{1^2.2^2.\lambda^4}-\frac{(2r)^6}{1^2.2^2.3^2.\lambda^6}+\&c.$$

Respecting the function f, it is only necessary to remark for my present purpose, that the vibrations that cause the sensation of light are only those for which r is very small compared to λ, so that only the first two terms of the above series are significant. The motion considered is, therefore, that along and immediately contiguous to the axis, which is distinguished from the rest of the motion by the analytical circumstance that the differential function $Vdz+wdr$ is more nearly an exact differential in proportion as r is less. (See Proposition X. in the communication to the Philosophical Magazine of December 1852.) If, now, an unlimited number of such vibrations, having the same values of λ, be propagated with their axes very close to each other and all parallel to the common direction of propagation, the transverse motions will destroy each other, and the result is simply a series of waves of longitudinal vibration and alternate condensation and rarefaction. For the sake of distinction, let us call the simple component vibrations *wave-rays*. By the principle of the coexistence of small vibrations, the *wave-rays* do not lose their individual character by being compounded in the manner just supposed. Hence if we would inquire what takes place when the series of compound waves impinges on a refracting medium, we must first ascertain what happens to each wave-ray considered by itself, the total effect being the sum of the effects on the separate rays. This question appears to admit of the following answer.

It will be assumed that the axis and all the parts of a wave-ray suffer refraction, on entering a medium, according to the

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ordinary law, and that the character of the ray is not altered in other respects. It will be necessary, first, to determine on this hypothesis the ratio of the condensation of a given portion of a given wave before intromittence, to the condensation of the same portion in the medium. For this purpose let us select a small rectangular portion, such that it has an edge, of length d_1, perpendicular to the plane of refraction, a second, of length d_2, parallel to the direction of propagation, and the third, which will be parallel to the plane front of the wave, of length d_3. Let s be the mean condensation of the rectangular parallelopipedon before intromittence into the medium, and s' its mean condensation after; and let d'_1, d'_2, d'_3 be the lengths of the edges in the latter case. Then by the law of refraction, ϕ and ϕ' being the angles of incidence and refraction,

$$\frac{d_1}{d'_1}=1, \text{ and } \frac{d_3}{d'_3}=\frac{\cos\phi}{\cos\phi'}.$$

Also, since a given number of waves without the medium are compressed after entering the medium within a space which is less than the space they occupied before in proportion to the less velocity of propagation, we have

$$\frac{d_2}{d'_2}=\frac{\sin\phi}{\sin\phi'}, \text{ and consequently } \frac{d_1 d_2 d_3}{d'_1 d'_2 d'_3}=\frac{\sin\phi\cos\phi}{\sin\phi'\cos\phi'}.$$

But the quantity of condensation remains the same, if we neglect the loss by reflexion, and the increment caused by the occupation of space by the atoms of the medium. Hence we shall have nearly,

$$\frac{s'}{s}=\frac{d_1 d_2 d_3}{d'_1 d'_2 d'_3}=\frac{\sin\phi\cos\phi}{\sin\phi'\cos\phi'}.$$

Consequently

$$s'=s\cdot\frac{\sin 2\phi}{\sin 2\phi'}.$$

In the particular case in which $s'=s$, ϕ and ϕ' are complements of each other, and μ being the index of refraction,

$$\mu=\frac{\sin\phi}{\sin\phi'}=\tan\phi.$$

According as ϕ is less or greater than this critical value, s' is greater or less than s.

Now supposing the wave-ray to be incident on the medium, reflexion necessarily takes place at the surface, on account of the *sudden* retardation which the vibrations there undergo by the aggregate effect of occupation of space by the atoms of the medium, and the obstacles they present to the free motion of the fluid. The reflexion, in this, as in every instance, is due to an

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abrupt change of condensation caused by a break of continuity of the circumstances of the fluid. So far as the sudden retardation acts in the direction of the axis of the wave-ray, it produces reflexion, but no polarization, because this action is symmetrical with respect to the axis. Reflexion also takes place by a sudden retardation of the *transverse* vibrations, because such retardation causes a sudden increment of condensation or rarefaction along the axis and contiguous parts. (The effect of transverse action is considered in a communication to the Philosophical Magazine for February 1853, under Proposition XIII.) If the transverse vibrations be resolved in directions perpendicular and parallel to the plane of refraction, no polarization appears if the retardations of the two sets of vibrations are equal. Polarization is in proportion to the *difference* of the retardations, because, if such difference exists, the action on the wave-ray is not symmetrical about its axis. The *plane* of polarization is coincident with the plane of refraction, because the retarding action is symmetrical about this plane.

These preliminaries being admitted, it may next be shown that polarization must be produced in every case of reflexion by oblique incidence. For the transverse vibrations perpendicular to the plane of refraction are suddenly impeded at the surface of the medium, without any opposite action in this direction, because, as we have seen, d_1 before refraction is the same as d'_1 after. As this is the case for every value of ϕ, this set of vibrations always gives rise to reflexion. The other set of transverse vibrations are also impeded, so far as the action of the medium is concerned; but simultaneously with the *increase* of condensation or rarefaction by this action, a *decrement* is caused by the change of d_3 into the greater value d'_3, and the consequent dispersion of the condensation or rarefaction over a larger space. The reflexion is due to the excess of the former action above the other. Hence the transverse vibrations of the reflected wave-ray perpendicular to the plane of reflexion, since they suffer no such diminution, are greater than those parallel to that plane; that is, the reflected ray is *partially polarized*.

In the particular case in which $\tan\phi=\mu$, it has been shown that $s'=s$. In this case the medium has no tendency to check either the longitudinal vibrations, or the transverse vibrations in the plane of reflexion. Consequently there is no reflexion of transverse vibrations in this plane, and the reflected ray is *completely polarized*. If this completely polarized ray be received at the same angle of incidence on the same substance, the plane of refraction being turned through 90°, as there will be no action to check either the direct or the transverse vibrations, there will be no reflexion whatever, as is known to be the fact.

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I am not aware that any other theory of light has accounted for complete polarization by reflexion, when the angle of incidence is such that $\tan \phi = \mu$.

It is evident from the above results that this theory gives the following answer to the question under consideration:—If the plane of reflexion of a ray completely polarized by reflexion be called the plane of polarization, its transverse vibrations are *perpendicular to the plane of polarization*.

Since the angle of incidence for complete polarization of a reflected ray is found by measurement to have the above value, which the theory gives on the hypothesis that the atoms of the refracting medium occupy a small space compared to the vacant space, the accordance of the theoretical result with fact is an incidental confirmation of that hypothesis.

Cambridge Observatory,
January 13, 1859.

XIV. *On the Rotation of Metallic Spheres by Electricity.* *By* George Gore, *Esq.* *From a letter addressed to* Dr. Bence Jones*.

IT has occurred to me that the phænomenon of the rolling ball, described in the Supplementary Number of the Philosophical Magazine of July last, forms a good illustration of the influence of the element *time* in the production of physical phænomena, which Mr. Faraday has so frequently insisted upon.

By reference to the description of the experiment, it will be found that the ball moves continuously in one uniform direction, and that it will move in either direction with equal facility. The explanation given of the motion in the article referred to, is that it appears to be due to an intermittent electro-thermic action taking place at the points of contact of the ball and rails at a minute distance behind the line of centre of gravity of the ball, *i. e.* that the passage of the electric current through the points of contact produces heat, and the heat causes motion by expanding into small protuberances the surfaces of the ball and rails at those points. It is evident that if the passage of the current, the heat, and the expansion, occurred *simultaneously*, without any period of time (however small) elapsing between them, the expansions would take place precisely under the centre of the ball, and the ball would have as great a tendency to move in one direction as in the other, and the progressive revolving motion would not be sustained. But if we suppose that a minute period of time is occupied in the production of the heat by the electricity, or of

* Communicated by Dr. Bence Jones.

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the expansion by the heat, or by both of these, then the phænomenon of motion (as obtained) becomes immediately intelligible; for during this period the ball has by its momentum moved forward a minute distance, and the expansion, instead of remaining simply a *lifting* power, becomes a *propelling* one by occurring at a small distance behind the line of centre of gravity of the ball; or, in other words, the maximum of expansion is always a little behind this point.

It has occurred to me, that, as the heat produced is that of conduction-resistance, the velocity of motion might perhaps be increased by using a ball composed of inferior conducting metal; and I have constructed balls similar in size to the one described, but composed of iron, also of nickel-silver. The iron ones were superior to those of copper, and the German-silver ones were better still. The nickel-silver balls invariably overtook the others if placed with them upon the same rails. A copper ball, weighing 376 grains, acted quite slowly, moving only about 19 feet per minute, while others of iron or of German-silver, weighing from 800 to 1000 grains each, moved at speeds varying from 30 to 42 feet per minute; and the increase of velocity by increase of conduction-resistance appeared to be capable of still further extension.

To further increase the velocity of motion, I have introduced into the apparatus the principle of action discovered by Peltier, termed electro-thermancy, by constructing balls composed of one hemisphere of iron and the other of nickel-silver, placing the iron half of the ball upon a nickel-silver rail, and the German-silver half upon an iron rail, and passing an electric current from the iron rail to the German-silver one, to generate heat of electro-thermancy in addition to that of conduction-resistance at the points of contact. In a number of experiments of this kind, I found that by first passing the current from the iron rail (through the ball) to the German-silver one in order to increase the heat, and then from the German-silver to the iron so as to decrease it, the velocities obtained were in the proportion of about $12\frac{1}{2}$ to 14; but this requires further confirmation.

It is highly necessary in comparative experiments to use balls that are smooth and perfectly free from oxide or greasy matter. Gilding the metals does not interfere with the action, and is an advantage by preventing oxidation.

8 Broad Street, Birmingham,
November 22, 1858.

Note to Mr. Gore's *Paper.*

The following appears to us to be an analysis of the interesting experiment so clearly described by Mr. Gore:—Suppose,

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first, the ball to be steady and the electric current to pass. Now push the ball forward—there is rupture of contact and a spark *behind* the new point of contact; a nipple suddenly emerges from the metal at the point where the spark occurs and pushes the ball forward. It is doubtless to this incessant rupture of contact that the *crackling* noticed by Mr. Gore is due. Mr. Gore confines his attention to *electric* conduction; but bad conductors of electricity are also bad conductors of heat—at least among the metals. That a bad conductor of heat produces a better effect than a good one, is a perfect illustration of the justice of Faraday's remark regarding the influence of bad conduction in the experiment of Trevelyan, namely, that it confines the heat to the neighbourhood of the surface, and therefore increases that local expansion which keeps up the motion.—J. T.

XV. *Note on the Stratifications of the Electric Light.* *By* Messrs. Quet *and* Seguin*.

As the cause of the luminous stratifications obtained with Ruhmkorff's induction apparatus is not yet known, it is perhaps not useless to endeavour to reproduce the phænomena from other sources of electricity, and to modify them by means of external agents. Messrs. Grove and Plücker have already tried the action of the magnet on the stratified light.

Stratifications obtained with the Electric Condenser.

If a Leyden jar be discharged through a cylindrical Geissler's tube, a wave of light is obtained which is usually dazzling, and in which stratifications are not observed. After the first discharge, it is easy to obtain two or three others, each of which gives a wave of stratified light throughout the length of the tube: the same phænomenon is obtained at the first discharge if the jar be feebly charged.

The luminous stratifications may be produced by converting the Geissler's tube into a condenser by means of a covering of tinfoil. The tube is charged like a Leyden jar, by passing the electricity of an ordinary plate machine, either into the very rarefied gas which it contains, or on to the armature of tin, the second conductor of the tube being placed in communication with the earth. The discharge of this apparatus produces in the tube a wave of stratified light; and strata are seen either on the covering of tinfoil, or on the part of the tube left exposed between the envelope and the electrode which is discharged on it. After the first, four or five other feebler discharges may be produced, all

* Translated by Dr. E. Atkinson, from the *Comptes Rendus*, December 13, 1858.

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of which exhibit the phænomena of luminous strata: the experiment also succeeds when the tinfoil is replaced by the hand. The electrophorus is sufficient to charge the tube; but when the machine is used, the armature of tin and one of the electrodes must be arranged so that the discharge takes place of itself, and thus the appearance of the wave of stratified light is frequently renewed. With a simple winding of metal wire applied on the tube instead of the tin, and communicating with the earth, some strata are produced.

Action of Conductors on Electric Currents, which produce either stratified or unstratified Light.

When the current of an induction coil is passed through a Geissler's tube, by connecting the two extremities of the induced wire with the electrodes of the tube, stratified light is immediately obtained. But if only single contact be made, and sparks drawn at the other electrode, there is obtained in the place of a stratified wave, a luminous wave without perceptible interruption, whose diameter is less than that of the tube. This stream continues to show itself always at the negative pole: sometimes it extends from one pole to the other; occasionally, besides the continuous wave, luminous strata are seen, which commence at a greater or less distance from the negative electrode and extend to the positive electrode. The length of the part of the tube occupied either by the continuous light or by the stratified light depends on the movement of the hammer, and on the density of the pile, on the force of the induction apparatus, and on the explosive distance from the pole of the tube; and the two discharges may be established at pleasure in the tube. With a feeble pile, and holding the hammer with the finger, the wave may be made continuous from one end to the other; by exerting on the hammer sufficient pressure, brilliant stratifications are produced with an almost dark space round the negative pole. Conductors brought near to the tube are not without influence on the two kinds of discharges, which Mr. Grove has already distinguished from one another.

When the stratified light is produced by the contact of two ends of the induced wire with two ends of the tube, if two fingers be applied to the tube so as to encircle it, or if it be covered with tinfoil communicating with the ground, it is seen that the luminous strata recede from each other in front of the conductor on the side of the positive pole, and at the edge of the conductor a very large dark space is produced. This effect is the more delicate the nearer the conductor is to the positive pole. It depends also on the force of the pile, and on the movement of the hammer. By supporting the hammer and using a feeble pile, the

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dark interval may attain a length of 6 centimetres. When the tinfoil or the fingers are caused to glide towards the positive pole, it appears as if the strata in front merge into each other, whereas they appear to recede from each other if the conductor moves towards the negative pole.

A very marked effect of conductors on the stratified light is obtained when the fluid of the inductive machine comes into the tube by one of the poles only, the other pole and the other end of the wire being insulated. The luminous strata are then very feeble; if the tube be encircled by the hand without touching, their diameter diminishes; they appear contracted towards the axis of the tube, but are more distinct. If the hand be pressed on the tube, or if a piece of tinfoil not insulated be affixed, the light becomes feeble between the conductor and the inactive electrode, but on the side of the active electrode the strata become more brilliant. At the same time, if the active electrode be positive, there appears on this side, at the edge of the tinfoil, a large dark space, as if this edge had become the negative pole.

The wave of continuous light, obtained by drawing sparks on one end of the tube while the other is in contact with the induced wire, is also influenced by external conductors. The hand which encircles without touching the tube, contracts the wave towards the axis. The contact of the fingers spreads it into a spindle; and if the pile be not very strong, the internal light appears to press against the glass in front of the external conductor. In the last case, the tube being simply taken between two opposite fingers, the continuous wave appears to undergo a disruption, and, further, not far from the positive pole, a brilliant layer is seen to be produced. By touching another section with two other fingers, a second rupture is effected, which produces the appearance of a second luminous stratum. Often the stratified wave which occupies that part of the tube near the positive pole, pushes the strata to a junction with those which the influence of the fingers has produced.

When only one of the poles is active, if the tinfoil which envelopes part of the tube be touched by the free extremity of the induced wire, the space situate between the tin and the inactive pole becomes much darker, and the other increases in brilliancy; at the same time there is produced on this part, that is, at the side of the active pole, a system of very complicated stratifications, which may be accounted for by supposing that each time that the hammer of the induction apparatus rises, two inverse and successive currents are propagated in the tube. The second of these currents is given by the electricity of the tinfoil, which rejoins by the induced wire that of the tube. By raising the

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hammer in the hand, the convexity of the strata is manifested sometimes in one sense, sometimes in the contrary, according as one or the other of these two currents predominates.

It is well to observe that the conductors are electrified by influence in these experiments. This is proved by insulating them, and making them communicate with an electroscope. Thus the tinfoil, when one of the poles is active and the second extremity of the induced wire is insulated, gives to the electroscope an electricity like that of the active pole. If the insulated extremity of the induced wire touches the tinfoil, the electroscope is charged with the electricity given by this wire to the conductor: the experiment must be made with attention, for the electricity appears alternately to approach and recede from the electroscope. This instrument may also be charged with the tinfoil when the two poles of the tube are active.

XVI. *A Proof that every Equation has as many Roots as it has Dimensions.* *By* Professor CHALLIS*.

IT will probably be conceded that this theorem, which has been the subject of so much discussion, will receive a legitimate proof, if a method, however operose, be indicated, by which as many roots of *any numerical equation* as there are dimensions can be actually found, whether they be possible or impossible, and at the same time it be shown that no other quantities are roots. It might perhaps be questioned whether any other kind of proof does not involve a *petitio principii*.

The method I am about to propose rests on the following algebraic principles. Algebra consists of two parts, the distinction between which is seldom marked with sufficient clearness in algebraic treatises. One part is wholly concerned with rules of operation and the generation of different kinds of symbolic representation of quantity, and is preparatory to, and independent of, the other part, which entirely consists in the formation and solution of equations. In the former the sign of equality means identity of the functions it separates, under difference of forms; in the latter the same sign means equality of value for certain values of an *unknown* quantity. These two significations might with advantage be distinguished by a difference in the sign. One of the most important and general results deducible from the operations of the first part of algebra is, that every function can be reduced to the form $A+B\sqrt{-1}$, A and B standing for algebraic functions which *en dernière analyse* are positive or negative numerical quantities. Now when in form-

* Communicated by the Author.

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ing an equation, x is put for the unknown quantity, and is operated upon by the rules of the first part of algebra, the necessary consequence is, that by the operations x acquires an algebraic expression. Hence to include every form that it can have, we may substitute for x in any equation $f(x)=0$, the general algebraic symbol $z+y\sqrt{-1}$. This substitution is legitimate also in another respect, because when the equation is thereby transformed into one of this form,

$$\phi(z, y^2)+y\,.\,\psi(z, y^2)\sqrt{-1}=0,$$

we must have separately,

$$\phi(z, y^2)=0 \text{ and } y\,.\,\psi(z, y^2)=0;$$

and thus there are two equations for determining the values of the two unknown quantities z and y. The second of these equations vanishes if $y=0$, in which case the other equation becomes identical with the original equation. But the value $y=0$ applies whenever the original equation has *possible* roots. This transformation, therefore, does not help us to find the possible roots of $f(x)=0$. As, however, this equation is by hypothesis *numerical*, there are always means of finding the possible roots. For instance, by substituting consecutive numerical values for x, separated by sufficiently small differences, and extending far enough positively and negatively, every possible value of x will be detected, and may by the ordinary modes of approximation be obtained as accurately as we please. When one such value α is found, the equation may be reduced one dimension by dividing by $x-\alpha$. As the equation may contain equal roots, it must be ascertained by trial whether the same quantity is a factor of derived equations. The number of equal roots is thus found, and the dimensions of the equation may be reduced accordingly. The same process must be gone through for all the other possible roots. Thus the residual equation, $\chi_1(x)=0$, will contain no possible root, and be of lower dimensions than the original equation by the number of possible roots in the latter. If $z+y\sqrt{-1}$ be now substituted for x in the equation $\chi_1(x)=0$, and the two resulting equations be

$$\phi_1(z, y^2)=0, \quad y\,.\,\psi_1(z, y^2)=0,$$

y can no longer be supposed zero, because in consequence of the preceding operations, $\chi_1(x)=0$ contains no possible roots. Hence y must have a possible value different from zero, and z a corresponding possible value such that the two values satisfy the equations $\phi_1(z, y^2)=0$ and $\psi_1(z, y^2)=0$, at the same time that $z+y\sqrt{-1}$ satisfies the equation $\chi_1(x)=0$. These are necessary consequences of the legitimate assumption, that the x of every equation stands for an algebraic expression. Hence after eliminating

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z from the above two equations, we may obtain a possible value of y^2 from the resulting equation by methods of approximation. Then substituting this value in one of the equations, we must obtain by the same methods, the corresponding possible value of z, such that $z+y\sqrt{-1}$ is found to satisfy the equation $\chi_1(x)=0$. The solution gives $+y$ and $-y$ simultaneously, because, as is known by other reasoning, impossible roots enter equations by pairs. Thus a *quadratic* factor of $\chi_1(x)=0$ is found, and the equation may be put under the form $Q_1 \cdot \chi_2(x)=0$. The equation $\chi_2(x)=0$ may be treated in exactly the same manner, and so on, till a number of quadratic factors be found equal to half the dimensions of $\chi_1(x)=0$. Hence if the dimensions be $2n$, we shall have $\chi_1(x) = Q_1 \cdot Q_2 \cdot Q_3 \ldots Q_n=0$. There cannot be *more* quadratic factors than these, because if there were, the dimensions of their product would exceed the dimensions of $\chi_1(x)$; and there cannot be *different* factors from these, because if any other quadratic factor divided $\chi_1(x)$, it must also divide $Q_1 \cdot Q_2 \cdot Q_3 \ldots Q_n$, which is impossible, because, not being identical with any one of these factors, it is prime to each. Consequently the number of the impossible roots is equal to the dimensions of $\chi_1(x)$; and the whole number of roots, possible and impossible, is equal to the dimensions of the given equation $f(x)=0$.

The complete solution of the equations $\phi_1(x, y^2)=0$ and $\psi_1(z, y^2)=0$, might give corresponding values of z and y, one or both of which might be impossible, and yet be such that $z+y\sqrt{-1}$ would satisfy the equation $\chi_1(x)=0$. It suffices for the foregoing argument, that there will always be one set at least of corresponding *possible* values of z and y, which will make $z+y\sqrt{-1}$ satisfy the same equation.

A method, practically possible, of finding as many roots of *any* proposed numerical equation as the equation has dimensions, having been indicated, and the impossibility of finding more having also been shown, it may be concluded generally that every equation has as many roots as dimensions.

Cambridge Observatory,
January 19, 1859.

XVII. *On the Hardness of Metals and Alloys.* *By* F. Crace Calvert, *M.R.A. of Turin, F.C.S. &c.; and* Richard Johnson, *F.C.S. &c.**

THE process at present adopted for determining the comparative degree of hardness of bodies, consists in rubbing one body against another, and that which indents or scratches the

* From the Memoirs of the Literary and Philosophical Society of Manchester, vol. xv. Session 1857–58.

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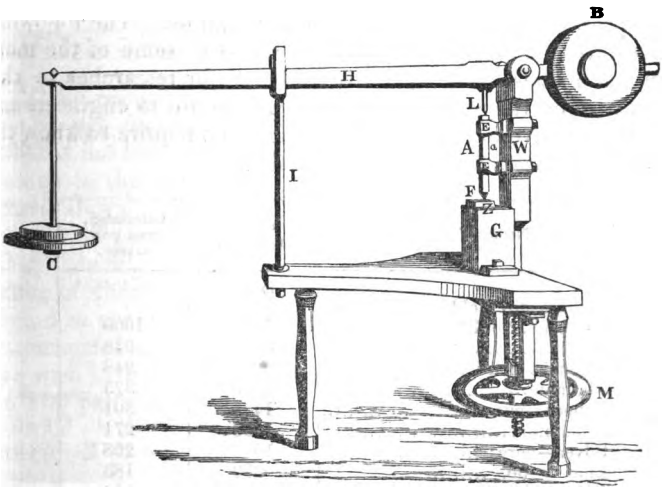
other is admitted to be the harder of the two bodies experimented upon. Thus, for example,

Diamond,	Iron,
Topaz,	Copper,
Quartz,	Tin,
Steel,	Lead.

This method is not only very unsatisfactory in its results, but it is also inapplicable for determining with precision the various degrees of hardness of the different metals and their alloys. We therefore thought that it would be useful and interesting if we were to adopt a process which would enable us to represent by numbers the comparative degrees of hardness of various metals and their alloys.

To carry out these views, we devised the following apparatus and method of operating. The machine used is on the principle of a lever, with this important modification, that the piece of metal experimented upon can be relieved from the pressure of the weight employed without removing the weight from the end of the longer arm of the lever. The machine consists of a lever, H, with a counterpoise, B, and a plate, C, on which the weights



are gradually placed. The fulcrum L bears on a square bar of iron A, passing through supports, E E. The bar A is graduated at *a*, and has at its end a conical steel point, F, 7 millims. or 0·275 of an inch long, 5 millims. or 0·197 of an inch wide at the base,

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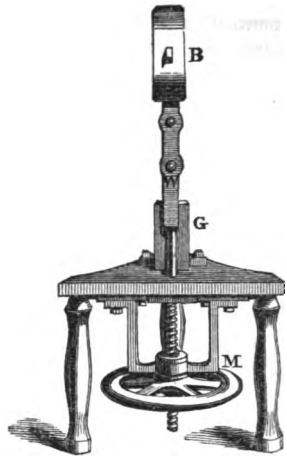
and 1·25 millim. or 0·049 of an inch wide at the point which bears on the piece of metal, Z, to be experimented on, and this is supported on a solid piece of iron, G. The support or point of resistance, W, is lowered or raised by the screw M; and when, therefore, this screw is turned, the whole of the weight on the lever is borne by the support I and the screw M. When it is necessary, by turning the screw M, the weight on the lever is re-established on the bar, and experimented upon.



When we wished to determine the degree of hardness of a substance, we placed it on the plate G, and rested the point F upon it, noticing the exact mark on *a* on the bar A, and then gradually added weights on the end of the lever, C, until the steel point, F, entered 3·5 millims. or 0·128 of an inch during half an hour, and then read off the weight. A result was never accepted without at least two experiments were made, which corresponded so far as to present a difference of only a few pounds. The following Table gives the relative degree of hardness of some of the more common metals. We specially confined our researches to this class, wishing the results to be practically useful to engineers and others who have to employ metals, and often require to know the comparative hardness of metals and alloys.

Names of metals.	Weight employed.	Calculated, cast iron =1000.
	lbs.	
Staffordshire Cold-blast Cast Iron —Grey, No. 3	4800	1000
Steel	4600?	958?
Wrought iron*	4550	948
Platinum	1800	375
Copper—pure	1445	301
Aluminium	1300	271
Silver—pure	1000	208
Zinc do.	880	183
Gold do.	800	167
Cadmium do.	520	108
Bismuth do.	250	52
Tin do.	130	27
Lead do.	75	16

* This wrought iron was made from the above-mentioned cast iron.

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This Table exhibits a curious fact, viz. the high degree of hardness of cast iron as compared with that of all other metals; and although we found alloys which possessed an extraordinary degree of hardness, still none were equal to cast iron.

The first series of alloys we shall give is that of copper and zinc.

Formulæ of alloys and per-centages.	Weight employed.	Obtained, cast iron = 1000.	Calculated*, cast iron = 1000.
	lbs.		
$Zn\,Cu^5$ { Cu 82·95 / Zn 17·05 }	2050	427·08	280·83
$Zn\,Cu^4$ { Cu 79·56 / Zn 20·44 }	2250	468·75	276·82
$Zn\,Cu^3$ { Cu 74·48 / Zn 25·52 }	2250	468·75	276·04
$Zn\,Cu^2$ { Cu 66·06 / Zn 33·94 }	2270	472·92	261·04
$Zn\,Cu$ { Cu 49·32 / Zn 50·68 }	2900	604·17	243·33
$Cu\,Zn^2$ { Cu 32·74 / Zn 67·26 }	Broke with 1500 lbs. without the point entering.		
$Cu\,Zn^3$ { Cu 24·64 / Zn 75·36 }	Broke with 1500 lbs. with an impression ¼ millim. deep.		
$Cu\,Zn^4$ { Cu 19·57 / Zn 80·43 }	Entered a little more than the above; broke with 2000 lbs.		
$Cu\,Zn^5$ { Cu 16·30 / Zn 83·70 }	Entered 2 millims. with 1500 lbs.; broke with 1700 lbs.		

These results show that all the alloys containing an excess of copper are much harder then the metals composing them, and what is not less interesting, that the increased degree of hardness is due to the zinc, the softer metal of the two which compose these alloys. The quantity of this metal must, however, not exceed 50 per cent. of the alloy, or the alloy becomes so brittle that it breaks as the steel point penetrates. We believe that some of these alloys, with an excess of zinc, and which are not found in commerce owing to their white appearance, deserve the attention of engineers. There is in this series an alloy to which we wish to draw special attention, viz. the alloy Cu Zn composed in 100 parts of—

Copper	49·32
Zinc	50·68
	100·00

* To calculate the hardness of an alloy, we multiplied the per-centage quantity of each metal by the respective hardness of that metal, added the two results together, and divided by 100. The quotient is the theoretical hardness.

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Although this alloy contains about 20 per cent. more zinc than any of the brasses of commerce, still it is, when carefully prepared, far richer in colour than the ordinary alloys of commerce. The only reason that we can give why it has not been introduced into the market is, that when the amount of zinc employed exceeds 33 per cent., the brass produced becomes so white that the manufacturers have deemed it advisible not to exceed that proportion. If, however, they had increased the quantity to exactly 50·68 per cent. and mixed the metals well, they would have obtained an alloy as rich in colour as if it had contained 90 per cent. of copper, and of a hardness three times as great as that given by calculation. In order to enable engineers to form an opinion as to the value of this cheap alloy, we give them the degrees of hardness of several commercial brasses:—

Commercial brasses.		Weight employed.	Cast iron = 1000.	
			Obtained.	Calculated.
		lbs.		
"Large Bearing"	Copper 82·05 *Tin 12·82 Zinc 5·13	2700	562	259
"Mud plugs" ...	Copper 80 *Tin 10 Zinc 10	3600	750	262
"Yellow Brass"	Copper 64 Zinc 36	2500	520	258
"Pumps and pipes"	Copper 80·0 *Tin 5·0 Zinc 7·5 Lead 7·5	1650	343	257

The alloy Cu Zn possesses another remarkable property, viz. the facility with which it is capable of crystallizing in prisms half an inch in length, of extreme flexibility. There is no doubt that this alloy is a definite chemical compound, and not a mixture of metals, as alloys are generally considered to be. Our researches on the conductibility of heat by alloys, which we have recently presented to the Royal Society, leave no doubt that many alloys are definite chemical compounds.

* These alloys all contain tin.

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On Bronze Alloys.

Formulæ of alloys and per-centages.	Weight employed.	Obtained, cast iron = 1000.	Calculated, cast iron = 1000.
	lbs.		
$Cu\,Sn^6$ { Cu 9·73 / Sn 90·27 }	400	83·33	51·67
$Cu\,Sn^4$ { Cu 11·86 / Sn 88·14 }	460	95·81	59·56
$Cu\,Sn^3$ { Cu 15·21 / Sn 84·79 }	500	104·17	68·75
$Cu\,Sn^2$ { Cu 21·21 / Sn 78·79 }	650	135·42	84·79
Cu Sn { Cu 34·98 / Sn 65·02 }	At 700 lbs. the point entered one-half, and the alloy broke.		
$Sn\,Cu^2$ { Cu 48·17 / Sn 51·83 }	At 800 lbs. the alloy broke without the point entering.		
$Sn\,Cu^3$ { Cu 61·79 / Sn 38·21 }	At 800 lbs. the alloy broke into small pieces (blue alloy).		
$Sn\,Cu^4$ { Cu 68·27 / Sn 31·73 }	1300 lbs. divided the alloy into two pieces without the point having entered 1 millim.		
$Sn\,Cu^5$ { Cu 72·90 / Sn 27·10 }	The same as the preceding.		
$Sn\,Cu^{10}$ { Cu 84·32 / Sn 15·68 }	4400	916·66	257·08
$Sn\,Cu^{15}$ { Cu 88·97 / Sn 11·03 }	3710	772·92	270·83
$Sn\,Cu^{20}$ { Cu 91·49 / Sn 8·51 }	3070	639·58	277·70
$Sn\,Cu^{25}$ { Cu 93·17 / Sn 6·83 }	2890	602·08	279·16

The results obtained from this series of alloys lead to several conclusions deserving our notice. First, the marked softness of all the alloys containing an excess of tin; secondly, the extraordinary fact that an increased quantity of so malleable a metal as copper should so suddenly render the alloy brittle, for the

Alloy $Cu\,Sn^2$

or

Copper . . . . . 21·21
Tin . . . . . 78·79 } is not brittle,

whilst the alloy Cu Sn

or

Copper . . . . . 34·98
Tin . . . . . 65·02 } is brittle.

Therefore the addition of 14 per cent. of copper renders a bronze alloy brittle. This curious fact is observed in all the alloys with excess of copper, $Sn\,Cu^2$, $Sn\,Cu^3$, $Sn\,Cu^4$, $Sn\,Cu^5$, until we arrive at one containing a great excess of copper, viz. the alloy $Sn\,Cu^{10}$, consisting of copper 84·68, and tin 15·32, when the brittleness ceases; but, strange to say, this alloy, which contains four-fifths

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of its weight of copper, is, notwithstanding, nearly as hard as iron. This remarkable influence of copper in the bronze alloys is also visible in those composed of

$Sn\,Cu^{15}$, containing 88·97 of copper.
$Sn\,Cu^{20}$, „ 91·49 „
$Sn\,Cu^{25}$, „ 93·17 „

Copper acquires such an increased degree of hardness by being alloyed with tin or zinc, that we thought it interesting to ascertain if alloys composed of these two metals would also have a greater degree of hardness than that indicated by theory: we accordingly had a series of alloys prepared in equivalent quantities; and these are the results arrived at:—

Formulæ of alloys and per-centages of each.	Weight employed.	Obtained, cast iron =1000.	Calculated, cast iron =1000.
	lbs.		
$Zn\,Sn^2$ { Zn 21·65, Sn 78·35 }	300	64·50	60·83
$Zn\,Sn$ { Zn 35·60, Sn 64·40 }	330	68·75	82·70
$Sn\,Zn^2$ { Sn 47·49, Zn 52·51 }	400	83·33	110·00
$Sn\,Zn^3$ { Sn 37·57, Zn 62·43 }	450	93·70	124·58
$Sn\,Zn^4$ { Sn 31·14, Zn 68·86 }	505	105·20	131·22
$Sn\,Zn^5$ { Sn 26·57, Zn 73·43 }	600	125·00	142·08
$Sn\,Zn^{10}$ { Sn 15·32, Zn 84·68 }	580	120·83	158·33

These results show that these metals exert no action on each other, as the numbers indicating the degrees of hardness of their alloys are rather less than those required by theory. Our researches on the conductibility of heat by the three above series of alloys throw, we believe, some light on the great difference which the alloys of bronze present as compared with those of tin and zinc; for we have stated above that the latter conduct heat as a mixture of metals would do, and not as the former series, which conduct heat as definite chemical compounds.

We shall conclude by giving the degrees of hardness of two other series of alloys, viz. those composed of lead and antimony, and lead and tin. In the series of lead and tin, we find that tin also increases the hardness of lead, but not in the same degree as it does that of copper.

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Lead and Antimony.

Formulæ of alloys and per-centages.	Weight employed.	
	lbs.	
$Pb\ Sb^5$ { Pb 24·31 / Sb 75·69 }	.....	Entered 2·5 millims. with 800 lbs.; then broke.
$Pb\ Sb^4$ { Pb 28·64 / Sb 71·36 }	.....	Entered 2·7 millims. with 800 lbs.; broke with 900 lbs.
$Pb\ Sb^3$ { Pb 34·86 / Sb 65·14 }	875	
$Pb\ Sb^2$ { Pb 44·53 / Sb 55·47 }	.....	Entered 2·5 millims. with 500 lbs.; broke with 600 lbs.
$Pb\ Sb$ { Pb 61·61 / Sb 38·39 }	500	
$Sb\ Pb^2$ { Pb 76·32 / Sb 23·68 }	385	
$Sb\ Pb^3$ { Pb 82·80 / Sb 17·20 }	310	
$Sb\ Pb^4$ { Pb 86·52 / Sb 13·48 }	300	
$Sb\ Pb^5$ { Pb 88·92 / Sb 11·08 }	295	

Lead and Tin.

Formulæ of alloys and per-centages.	Weight employed.	Obtained, cast iron = 1000.	Calculated, cast iron = 1000.
	lbs.		
$Pb\ Sn^5$ { Pb 26·03 / Sn 73·97 }	200	41·67	23·96
$Pb\ Sn^4$ { Pb 30·57 / Sn 69·43 }	105	40·62	23·58
$Pb\ Sn^3$ { Pb 36·99 / Sn 63·01 }	160	32·33	22·83
$Pb\ Sn^2$ { Pb 46·82 / Sn 53·18 }	125	26·04	20·09
$Pb\ Sn$ { Pb 63·78 / Sn 36·22 }	100	20·83	19·77
$Sn\ Pb^2$ { Pb 77·89 / Sn 22·11 }	125	26·04	18·12
$Sn\ Pb^3$ { Pb 84·09 / Sn 15·91 }	135	28·12	17·23
$Sn\ Pb^4$ { Pb 87·57 / Sn 12·43 }	125	26·04	17·08
$Sn\ Pb^5$ { Pb 89·80 / Sn 10·20 }	110	22·92	16·77

We have great pleasure in thanking here Mr. Siméon Stoikowitsch, F.C.S., for his valuable assistance during these long researches.

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XVIII. *On Iodo-arsenious Acid.*

By WILLIAM WALLACE, *Ph.D., F.C.S., Glasgow**.

THE results of a recent investigation on the compound of arsenious acid with chloride of arsenic, to which I gave the name of chloro-arsenious acid†, have induced me to prosecute the inquiry whether similar combinations could be formed containing bromine and iodine. I have succeeded in obtaining a bromo-arsenious acid having a remarkably close resemblance to the chlorine compound; but as my experiments on this interesting body are not yet completed, I reserve a description of them for a future communication.

The iodine compound differs very much from those containing chlorine and bromine. I find that it has already been described as an arsenite of teriodide of arsenic by Plisson, and also by Serullas and Hottot ‡; but neither of these authorities have given any analysis of it, nor do they appear to have understood its true nature.

Iodide of arsenic is readily obtained by heating iodine with an excess of powdered metallic arsenic, and distilling or subliming the compound. It forms, as thus prepared, a brick-red crystalline mass; but when distilled in an atmosphere of hydrogen, it is obtained of a yellowish-red colour, while a small portion of arsenic is set free, arising from the presence of traces of arsenious acid in the iodide.

The iodide requires for solution at the boiling temperature 3·32 parts of water. On boiling down this liquid, beautiful red-coloured crystals, consisting of pure and anhydrous iodide of arsenic, are obtained.

When, however, the solution is allowed to cool slowly, thin pearly scales gradually separate: this compound is the subject of the present communication. It cannot be washed with water without partial decomposition, and is best dried by pressure between folds of bibulous paper. When freed from the adhering red mother-liquor it is quite colourless, but it acquires a slight yellow tint on exposure to the air, owing to the separation of a minute quantity of iodine. On being strongly heated, it yields a sublimate consisting chiefly of teriodide of arsenic, while grey-coloured arsenious acid remains behind. The compound contains chemically combined water, which it loses completely over oil of vitriol. It cannot be dissolved in water without undergoing decomposition.

* Communicated by the Author.

† "On Chloro-arsenious Acid and some of its Compounds," Phil. Mag. November 1858.

‡ *Vide* Gmelin's 'Handbook,' vol. iv.

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Two quantities of the compound were prepared, and submitted to careful analysis, after desiccation over oil of vitriol. The following results were obtained:—

	I.	II.		
Arsenic . .	57·90	58·7	4=300	58·25
Iodine . . .	25·19	24·6	1=127	24·66
Oxygen . .	...	...	11= 88	17·09
			515	100·00

The crystals consist therefore of iodo-arsenious acid, combined with 3 equivs. of arsenious acid, $AsIO^2$, $3AsO^3$. They may also be regarded as a compound of teriodide of arsenic with 11 equivs. of arsenious acid, AsI^3, $11AsO^3$; but this constitution is not by any means a probable one.

A portion of the crystals, dried as completely as possible between folds of bibulous paper, lost 19 per cent. by desiccation over oil of vitriol. This agrees with 12 equivs. of water, giving for the composition of the crystals the formula

$$AsIO^2, 3AsO^3 + 12HO, \text{ or } AsIO^2, 3HO + 3(AsO^3, 3HO).$$

When a large excess of hydriodic acid is present in the solution of iodide of arsenic in boiling water, the crystals that separate on cooling consist, not of the compound described above, but of pure iodide of arsenic. It appears, therefore, that iodo-arsenious acid cannot be obtained except in combination with arsenious acid.

I have endeavoured to form compounds with iodide of ammonium and iodide of potassium, but without success. The addition of either of these salts to a cold saturated solution of iodide of arsenic, causes the formation of pearly crystals having the same composition as those obtained by cooling a hot saturated solution; while boiling-down gives rise to the separation of the teriodide.

XIX. *On* Poinsot's *four new Regular Solids.*

By A. Cayley, *Esq.**

IT is shown by Poinsot, in the "Mémoire sur les Polygones et les Polyèdres," *Jour. Polyt.* vol. iv. pp. 16 to 48 (1810), that, besides the regular polyhedrons of ordinary geometry, there are (of course in an extended signification of the term) four new regular polyhedrons, viz. an icosahedron, which I will call the great icosahedron (No. 33 of the Memoir), and three dodecahedrons, which I will call the great dodecahedron (No. 37), the great stellated dodecahedron (No. 38), and the small stellated dodecahedron (No. 39). The nature of Poinsot's genera-

* Communicated by the Author.

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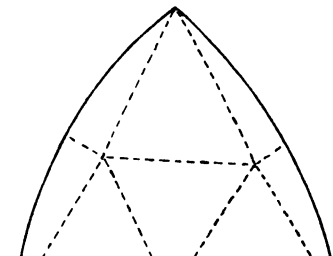
lization will be best understood by conceiving, as he does, that the polyhedron is projected on a concentric sphere, so that the faces become spherical polygons. Then for the ordinary polyhedrons of geometry, the sum of the angles at a vertex = 4 right angles; but it may, according to the more general notion, be = e times 4 right angles. In like manner for the ordinary polyhedrons, the sides of a face subtend at the centre angles, the sum of which is = 4 right angles; but according to the more general notion, this sum may be (viz. if the polygons are stellated) = e' times four right angles. And finally, the sum of the spherical polygons is ordinarily equal to the entire spherical surface; but according to the more general notion, it may be = D times the spherical surface. (e is Poinsot's e; e' does not occur in Poinsot; and, for a reason which will appear, I have written D for Poinsot's E.)

The new polyhedra are constructed as follows:—

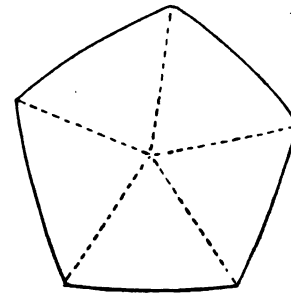
1. *The great Icosahedron.*—Each face is made up of seven faces, or rather four faces and six half faces of the ordinary icosahedron, in the manner shown by fig. 1. There are, as in the ordinary icosahedron, five angles at each vertex; but these make up together, not four, but eight right angles, or $e = 2$; but, as in the ordinary polyhedra, $e' = 1$; and the sum of all the faces is obviously seven times the spherical surface, or D = 7. (Also E = 7.)

Fig. 1.



2. *The great Dodecahedron.*—Each face is made up of five faces of the ordinary icosahedron in the manner shown by the figure 2. There are five angles at each vertex, and these make up together eight right angles, or $e = 2$; but, as in ordinary polyhedra, $e' = 1$; and the sum of all the faces is obviously $12 \times \frac{5}{20}$, or three times the spherical surface, or D = 3. (Also E = 3.)

Fig. 2.



3. *The great stellated Dodecahedron.*—Each face is formed by stellating a face of the great dodecahedron in the manner

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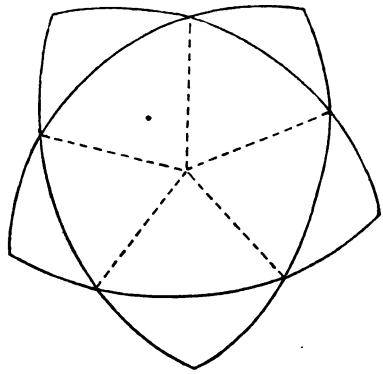
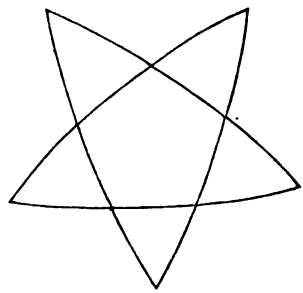
shown by fig. 3. There are, as in the ordinary dodecahedron, three angles at each vertex, and the sum of these is simply four right angles, or $e=1$. On account of the stellation, $e'=2$. Each of the projecting parts of the face is equal $\frac{1}{5}$ of the face of the ordinary icosahedron; and if we reckon the area of the stellated pentagon to be that of the interior pentagon *plus* the projecting parts, the area of the face will be $5+\frac{5}{5}$, or $\frac{20}{3}$ of the face of the ordinary icosahedron; and the sum of the faces will be four times the spherical surface, and accordingly Poinsot writes $E=4$. If, however, what seems preferable, we reckon the area of the stellated pentagon as five times the triangle, having for its vertex the centre of the face and standing upon a side (or what is the same thing, reckon the stellated pentagon as *twice* the interior pentagon *plus* the projecting parts), then the area of the face will be $10+\frac{5}{5}$ or $\frac{55}{5}$ of the face of the ordinary icosahedron, and the sum of the faces will be seven times the spherical surface, or $D=7$.

Fig. 3.



4. *The small stellated Dodecahedron.*—Each face is formed by stellating a face of the ordinary dodecahedron, as shown by fig. 4. There are five angles at each vertex; and the sum of these is four right angles, or $e=1$. On account of the stellation, $e'=2$. The area of each of the projecting parts is $\frac{1}{5}$ of the interior pentagon or face of the ordinary dodecahedron; and, according to the first mode of measurement, the area of the stellated face is twice that of the face of the ordinary dodecahedron, and the sum of the faces is twice the spherical surface, and accordingly Poinsot writes $E=2$. But according to the second mode of measurement, the area of the stellated pentagon is three times that of the face of the ordinary dodecahedron, and the sum of the faces is three times the spherical surface, or we have $D=3$.

Fig. 4.



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I form now the following Table, comprising as well the ordinary five figures as the new ones of Poinsot, and where we have

H, the number of faces.
S, the number of vertices.
A, the number of edges.
n, the number of sides to a face.
n', the number of sides (angles) at a vertex.
e, viz. the angles at a vertex make together e times four right angles.
e', viz. the angles which the sides of a face subtend at the centre of the face make together e' times four right angles.
E, viz. the faces make together E times the spherical surface, the area of a stellated face being reckoned (as by Poinsot), each portion being taken once only.
D, viz. the faces make together D times the spherical surface, the area of a stellated face being reckoned as the sum of the triangles, having their vertices at the centre of the face and standing on the sides.

The Table is—

Designation.	H.	S.	A.	n.	n'.	e.	e'.	D.		E.
Tetrahedron	4	4	6	3	3	1	1	1		1
Hexahedron	6	8	12	4	3	1	1	1		1
Octahedron	8	6	12	3	4	1	1	1		1
Dodecahedron	12	20	30	5	3	1	1	1		1
Icosahedron	20	12	30	3	5	1	1	1		1
Great stellated dodecahedron	12	20	30	5	3	1	2	7		4
Great icosahedron	20	12	30	3	5	2	1	7		7
Small stellated dodecahedron	12	12	30	5	5	1	2	3		2
Great dodecahedron	12	12	30	5	5	2	1	3		3

where the figures which are polar reciprocals of each other are written in pairs: viz. as is well known, the tetrahedron is its own reciprocal, the hexahedron and octahedron are reciprocals, and the dodecahedron and icosahedron are reciprocals; moreover the great stellated dodecahedron and the great icosahedron are reciprocals, and the small stellated dodecahedron and the great dodecahedron are reciprocals. The number which I have

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called D is reciprocal to itself; this is not the case for Poinsot's E; and I have not been able to define E in such a manner as to enable me to form the definition of a reciprocal number E′: this may be possible, but in the mean time it seems better to discard E altogether, and use instead of it the number D.

Euler's well-known relation applying to ordinary polyhedra is

$$S+H=A+2.$$

Poinsot in his memoir has (by an extension of Legendre's demonstration of Euler's theorem) obtained the more general relation,

$$eS+H=A+2E,$$

which, however, does not apply to the two stellated figures where e' is different from unity; the general form is

$$eS+e'H=A+2D,$$

which applies to all the nine figures. This applies to all polyhedra, regular or not, which are such that e has the same value for each vertex, and e' the same value for each face. To prove it, we have only to further extend Legendre's demonstration. If for any face, stellated or not, the sum of the angles is s, and the number of sides n, then, according to the foregoing mode of reckoning, the area of the face (measured in right angles) is

$$s+4e'-2n.$$

Now the sum of all the faces is D times the spherical surface, $=8D$. But the sum of the term s is equal to the sum of the angles about each vertex, $=4eS$; the sum of the term $4e'$ is $=4e'H$, the sum of the term $2n$ is four times the number of edges, $=4A$. Hence $4eS+4e'H-4A=8D$, or $eS+e'H=2D$.

I remark that the small stellated dodecahedron and the great dodecahedron are descriptively the same figures, and that, if we represent the vertices by $a, b, c, d, e, f, g, h, i, j, p, q$, and the faces by A, B, C, D, E, F, G, H, I, J, P, Q, then the relations of the vertices and faces is shown by either of the following Tables:—

$a\ b\ c\ d\ e = P,$	A C E B D $= p,$
$p\ b\ i\ h\ e = A,$	P I E B H $= a,$
$p\ e\ j\ i\ a = B,$	P J A C I $= b,$
$p\ d\ f\ j\ b = C,$	P F B D J $= c,$
$p\ e\ g\ f\ c = D,$	P G C E F $= d,$
$p\ a\ h\ g\ d = E,$	P H D A G $= e,$
$j\ c\ d\ g\ q = F,$	J D Q C G $= f,$
$f\ d\ e\ h\ q = G,$	F E Q D H $= g,$
$g\ e\ a\ i\ q = H,$	G A Q E I $= h,$
$h\ a\ b\ j\ q = I,$	H B Q A J $= i,$
$i\ b\ c\ f\ q = J,$	I C Q B F $= j,$
$f\ g\ h\ i\ j = Q.$	F H J G I $= q.$

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where it is to be noticed that in either Table each non-consecutive duad of any pentad occurs once, and only once, as a non-secutive duad of another pentad. The restriction that a non-consecutive duad of any multiplet is *not* to occur as a duad, consecutive or non-consecutive, of any other multiplet (see my note appended to Mr. Kirkman's paper "On Autopolar Polyhedra," Phil. Trans. 1857, p. 183), applies only to ordinary polyhedra, and not to the class here considered.

2 Stone Buildings, W.C.,
January 13, 1859.

XX. *On Apatite, Wagnerite, and some artificial species of Metallic Phosphates.* *By* Messrs. H. Sainte-Claire Deville *and* H. Caron*.

AMONG the more abundant minerals is found a well-defined and crystallized substance, the phosphate of lime, which is principally met with in veins in the older rocks and in volcanic lavas. The singular composition of apatite, first determined by M. Gustav Rose in 1827, shows that it is a definite compound of chloride and fluoride of calcium with phosphate of lime. The chemical examination of this substance, and the establishment of its analogies, formed an interesting subject of inquiry; and the researches which we have undertaken have led to results of great simplicity.

By the side of apatite is placed another mineral, Wagnerite, composed of the same or analogous elements, but in different proportions. The calcium of the apatite is replaced by magnesium; in addition to which, apatite is a regular hexagonal prism, while Wagnerite is an oblique rhomboidal prism. Their form and composition thus distinguish them from each other, and we shall show that each may be considered as the type of two groups of which we have established all the species.

Apatite has the composition

$$3(PO^5, 3CaO)\left\{\begin{matrix}Cl\\F\end{matrix}\right\}Ca;$$

Wagnerite is represented by the simpler formula,

$$(PO^5, 3MgO)\left\{\begin{matrix}Cl\\F\end{matrix}\right\}Mg.$$

We have prepared the apatites and Wagnerites which form the species of these two groups, and which are comprised in the following list:—

* From the *Comptes Rendus*, December 20, 1858.

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Apatites.

	Composition.	Mineralogical names.
Lime apatite . . .	$3(PO^5, 3CaO)(Cl\,Ca)$.	Apatite.
Lead apatite . . .	$3(PO^5, 3PbO)(Cl\,Pb)$.	Pyromorphite.
Baryta apatite. . .	$3(PO^5, 3BaO)(Cl\,Ba)$.	Artificial species.
Strontian apatite . .	$3(PO^5, 3SrO)(Cl\,Sr)$.	Artificial species.

Wagnerites.

Magnesia Wagnerite.	$(PO^5, 3MgO)(Cl\,Mg)$.	Wagnerite.
Lime Wagnerite . .	$(PO^5, 3CaO)(Cl\,Ca)$.	Artificial species.
Manganese Wagnerite	$(PO^5, 3MnO)(Cl\,Mn)$.	Artificial species.
Wagnerite of iron and manganese . .	$\left\{PO^5, 3\left(\begin{matrix}Mn\\Fe\end{matrix}O\right)\right\}\left\{Cl\left(\begin{matrix}Mn\\Fe\end{matrix}\right)\right\}$.	Zwieselite.

In these bodies we have been able to replace a part or even the whole of the chlorine by fluorine, without in general changing the crystalline form, showing in this case the isomorphism of fluorine and chlorine, which hitherto has only been strictly demonstrated in a few cases.

It will be observed that the bases in apatite are such metallic oxides as, in combining with carbonic acid, give rhombic carbonates of the same form as Arragonite; the Wagnerites, on the other hand, are exclusively composed of such metallic oxides as give, when combined with carbonic acid, rhombohedral carbonates, or spars of the same form as calcareous spar. To complete this singular comparison, it will be remarked that carbonate of lime is dimorphous, crystallizing either in rhombic prisms (Arragonite) or in rhombohedra (calcareous spar). Hence the lime officiates as intermediate agent, or *pivot* as it has been elsewhere* called, between these two groups of metallic oxides. It is the same here. We have obtained a calcareous Wagnerite, hitherto unknown, by replacing, either wholly or partially, magnesia by lime, and fluorine by chlorine. This calcareous Wagnerite has therefore the composition

$$PO^5, 3CaO\,(Cl\,Ca),$$

which has been confirmed by analysis, and which approximates it to the second group of chlorophosphates.

Moreover, all our efforts to obtain Wagnerite from purely *Arragonitic* oxides, and apatites from purely *spathic* oxides, have been unfruitful, so that the two divisions of the metallic carbonates appear again in the phosphates; but here we not only find incompatible crystalline form, but also different composition.

M. Daubrée† has prepared apatite by passing chloride of phos-

* *Comptes Rendus*, vol. xxxviii. p. 401.

† *Annales des Mines*, 4 sér. vol. xix. p. 654.

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phorus over lime; M. Manross* and M. Briegleb†, in continuation of the remarkable researches made in the laboratory of Professor Wöhler, have reproduced apatite in more distinct and more beautiful forms by taking advantage of the double decomposition of alkaline phosphates and chloride of calcium; M. Forchhammer‡, by acting on phosphate of lime with chloride of sodium, has obtained very beautiful specimens of the same mineral species.

We employed a more direct and more general method, founded on the fact that the phosphates are soluble at a red heat in the chlorides of the metals whose oxides serve as base in the salts employed, or in analogous chlorides. Thus by taking bone-phosphate, and mixing it with sal-ammoniac to transform the carbonate of lime, which it always contains, into chloride of calcium, and adding an excess of chloride and fluoride of calcium, we obtain by fusion at a red heat a fluid which appears homogeneous, and from which apatite crystallizes§ on cooling. It is well to operate as much as possible with crucibles or vessels of graphite from gas-retorts; for the phosphates strongly attack clay crucibles: the phosphate of lime may be replaced by any of the phosphates mentioned. This may be prepared for the purpose by calcining one equivalent of commercial phosphate of ammonia with three equivalents of the oxide or nitrate of the metal in question. The salt obtained is mixed with the coresponding chloride and heated. On cooling, the excess of chloride is separated by washing with distilled water. Iron apatite (Zwieselite) is obtained by treating phosphate of iron with chloride of manganese, a process which yields it in crystals frequently a centimetre long.

The exact determination of the crystalline form of Wagnerite is difficult on account of the numerous striæ in the facets, more especially of those zones the most easily measured We must further observe that the phosphates retain fluorine with such persistence as to lead to most serious errors if the analysis be not made very carefully.

The occurrence of apatite in veins has led M. Daubrée to think that this substance may have been conveyed into its position in the form of volatile products, and in particular by the action of chloride of phosphorus on lime,—a reaction which in fact produces apatite, for it brings together chloride of calcium and phosphate of lime. The presence of fluorine would be more difficult to explain on this supposition; but an observation we have made renders the hypothesis of M. Daubrée admissible

* Experiments, &c., Inaugural Thesis. Göttingen, 1852.
† Liebig's *Annalen*, vol. xcviii. p. 95. ‡ Ibid. vol. xc. p. 77.
§ This beautiful substance resembles the apatite of the Vesuvian lavas.

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under much simpler conditions. In fact apatites and Wagnerites become volatile, at a slightly elevated temperature, in the vapour of the metallic chlorides in the midst of which they are formed. We have thus been able to distil at a red heat Wagnerite in the vapour of chloride of magnesium; and the volatilized crystals, which were analysed, had the constitution of the primitive substance. Apatite also volatilizes in the vapour of chloride of calcium; and by using carbon vessels we have obtained beautiful crystals of sublimed apatite. This singular phænomenon may be classed with certain well-known facts, such as the volatilization of boracic acid in aqueous vapour, of sulphide of boron in sulphuretted hydrogen, &c. It appears evident that these phænomena are not purely mechanical; and when they have been studied, they may contribute to explain various facts of nature.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 72.]

June 17, 1858.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Action of Bichloride of Carbon on Aniline." By A. W. Hofmann, Ph.D., F.R.S.

In two former notes I have described the deportment of aniline as the prototype of primary monamines with the bromine- and chlorine-compounds of biatomic and triatomic radicals. It was found that under the influence of these agents, two equivalents of aniline coalesce to a more complex molecule, retaining the chemical character of the constituents; the action of bibromide of ethylene and chloroform producing respectively—

Diethylene-diphenyl-diamine $C_{32}H_{18}N_2 = \left\{ \begin{matrix} (C_4H_4)_2'' \\ (C_{12}H_5)_2 \end{matrix} \right\} N_2$.

Formyl-diphenyl-diamine .. $C_{26}H_{12}N_2 = \left\{ \begin{matrix} (C_2H)''' \\ (C_{12}H_5)_2 \\ H \end{matrix} \right\} N_2$.

The result of these experiments led me to examine the behaviour of aniline under the influence of organic chlorides containing even a larger number of chlorine equivalents. The agent selected was the body well known by the name of bichloride of carbon, *i. e.* tetrachlorinetted marsh-gas, or chloroform, in which the residuary equivalent of hydrogen is replaced by chlorine.

Aniline and bichloride of carbon do not act upon each other at the common temperature; at the temperature of boiling water a change is perceptible, but even after several days' exposure the reaction is far from being complete. On submitting, however, a mixture of $3\frac{1}{2}$ parts by weight of aniline and 1 part of bichloride of carbon,

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both in the anhydrous state, for about thirty hours to a temperature of 170° C., the liquid will be found to be converted into a black mass, either soft and viscid, or hard and brittle, according to time and temperature.

This black mass, which adheres firmly to the tubes in which the reaction has been accomplished, is a mixture of several bodies. On exhausting with water, a portion dissolves, while a more or less solid resin remains behind.

The aqueous solution yields, on addition of potassa, an oily precipitate containing a considerable portion of unchanged aniline; on boiling this precipitate with dilute potassa in a retort, the aniline distils over, whilst a viscid oil remains behind, which gradually solidifies with a crystalline structure. Washing with cold alcohol and two or three crystallizations from boiling alcohol render this body perfectly white and pure, a very soluble substance of a magnificent crimson colour remaining in solution.

The portion of the black mass which is insoluble in water dissolves almost entirely in dilute hydrochloric acid, from which solution it is reprecipitated by the alkalies in the form of an amorphous pink or dingy precipitate soluble in alcohol with a rich crimson colour. The greater portion of this body consists of the same colouring principle which accompanies the white crystalline substance. On the other hand, considerable quantities of this crystalline body are occasionally present in the product insoluble in water.

The crystalline body is insoluble in water, difficultly soluble in boiling alcohol, soluble in ether. From the hot alcoholic solution it crystallizes slowly on cooling in elongated four-sided plates, often grouped round a common centre; this substance is a well defined base; it freely dissolves in acids, from which, on the addition of the alkalies, it is thrown down as a dazzling white precipitate.

The analysis of this new base has led to the expression

$$C_{38}H_{17}N_3,$$

a formula corroborated by the analysis of a fine, somewhat difficultly soluble hydrochlorate,

$$C_{38}H_{17}N_3, HCl,$$

which is obtained by dissolving an excess of the new base in hot diluted hydrochloric acid, when it crystallizes on cooling.

A further confirmation was furnished by the examination of a bright yellow platinum-salt,

$$C_{38}H_{17}N_3, HCl, PtCl_2.$$

Both the hydrochlorate and the platinum-salt are extremely soluble in an excess of hydrochloric acid, which has therefore to be carefully avoided in their preparation.

The phase of the action of bichloride of carbon on aniline, which gives rise to the formation of the new base, is expressed by the equation

$$6C_{12}H_7N + C_2Cl_4 = C_{38}H_{17}N_3, HCl + 3(C_{12}H_7N, HCl).$$

What is the constitution of the new body? It is obviously derived

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from 3 molecules of aniline from which 4 equivalents of hydrogen have been eliminated by the 4 equivalents of chlorine in the bichloride, the carbon entering as a biatomic molecule into the complex atom. The new body would thus become a triamine,

$$\left.\begin{matrix} C_2'' \\ (C_{12}H_5)_3 \\ H_2 \end{matrix}\right\} N_3.$$

It is however more probable that the carbon replaces in the form of cyanogen, when the new compound appears in the light of a diamine, as

$$\text{Cyan-triphenyl-diamine } \left.\begin{matrix} C_2N \\ (C_{12}H_5)_3 \\ H_2 \end{matrix}\right\} N_2.$$

The new compound thus becomes closely allied to melaniline, which may be viewed as diphenyl-cyan-diamine,

$$\text{Melaniline } C_{26}H_{13}N_3 = \left.\begin{matrix} C_2N \\ (C_{12}H_5)_2 \\ H_3 \end{matrix}\right\} N_2.$$

It deserves to be noticed, that in its appearance, and in its general characters, cyan-triphenyl-diamine resembles melaniline in a remarkable manner.

If we are entitled to view the new body which forms the subject of this note as a cyanogen-substitute, we have not less than four well-defined diamines of the phenyle-series.

$$\text{Diethylene-diphenyl-diamine } \left.\begin{matrix} (C_4H_4)''_2 \\ (C_{12}H_5)_2 \end{matrix}\right\} N_2.$$

$$\text{Formyl-diphenyl-diamine .... } \left.\begin{matrix} (C_2H)''' \\ (C_{12}H_5)_2 \\ H \end{matrix}\right\} N_2.$$

$$\text{Cyan-diphenyl-diamine ..... } \left.\begin{matrix} C_2N \\ (C_{12}H_5)_2 \\ H_3 \end{matrix}\right\} N_2.$$

$$\text{Cyan-triphenyl-diamine..... } \left.\begin{matrix} C_2N \\ (C_{12}H_5)_3 \\ H_2 \end{matrix}\right\} N_2.$$

I intend to continue the inquiry still further in this direction, and propose next to examine the deportment of aniline with the so-called protochloride (C_4Cl_4) and sesquichloride of carbon (C_4Cl_6).

"Researches on the Phosphorus-Bases." By A. W. Hofmann, Ph.D., F.R.S.

In a paper published in the Transactions of the Royal Society, we (M. Cahours and myself) have given a detailed account of the preparation of the phosphorus-bases, and also an accurate description of triethylphosphine, the most characteristic and accessible representative of this class of compounds.

The object of our joint inquiry was chiefly to examine the phosphorus-bases as a class, and to establish their analogy with the corre-

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sponding terms of the nitrogen-series. The deportment of the phosphorus-bodies in their relation to other compounds has as yet been scarcely investigated. For several months I have been engaged in this study, which promises a rich harvest of results. Most of the experiments were made with triethylphosphine, a substance which, in consequence of its convenient position in the system of organic compounds, in consequence of the variety of its attachments, the energy and precision of its action, and, lastly, the well-defined character of its compounds, will probably become an agent of predilection in the hands of the chemist.

It is my intention to trace the history of this remarkable body in its several directions; and for this purpose, in fact, a considerable amount of material has been already accumulated. But since necessarily some time must elapse before such an inquiry, which from the peculiar character of the compound is often obstructed by unusual difficulties, can be completed, I beg leave to present my results in the same measure as the inquiry advances, hoping that at a later period I may be allowed to collect the scattered observations, and to lay them in a more elaborated and digested form before the Society.

Among the numerous reactions of triethylphosphine, my attention has been chiefly directed to the compounds which this body furnishes when submitted to the action of organic chlorides, bromides, and iodides.

I. *Action of Bibromide of Ethylene upon Triethylphosphine.*

In the anhydrous condition the two bodies act even at the common temperature with considerable power upon each other, a white crystalline substance being immediately precipitated. If the reaction be allowed to go on in the presence of a large volume of anhydrous ether, the deposition of the crystalline body is considerably retarded, unless the mixture, in an appropriate apparatus, be exposed to the temperature of boiling water. After a short digestion, on distilling off the ether and the excess of bibromide, a crystalline cake is left in the retort, consisting of several bromides, the nature and the relative proportions of which appear in a great measure to depend upon the rapidity of the reaction. I have found it most convenient to work with ethereal solutions at the common temperature.

The determination of the bromine in the crystalline body revealed at once the compound character of this substance, for it steadily diminished by dissolving the bromide in absolute alcohol, and reprecipitating it partially by ether. By repeating this process four or five times, a body of constant composition was obtained.

The compound thus prepared is a crystalline mass, without odour, extremely soluble in water, and even in absolute alcohol, but insoluble in anhydrous ether. It exhibited a rather unexpected composition, for on analysis it was found to contain

$$C_{16}\,H_{19}\,PBr_2,$$

and consequently to have been formed by the simple union of 1 equiv.

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of triethylphosphine and 1 equiv. of bibromide of ethylene,

$$C_{12}H_{15}P + C_4H_4Br_2 = C_{16}H_{19}PBr_2.$$

The bromine in this compound exists in two perfectly different forms; addition of nitrate of silver precipitated only one-half of this element as bromide of silver, while even by protracted ebullition the second half remained untouched. The result changed, however, on digestion with freshly precipitated oxide of silver, when the whole of the bromine separated at once in the form of bromide of silver.

On adding to the solution of the bromide an excess of nitrate of silver, filtering off the bromide, and removing the excess of silver by hydrochloric acid, a corresponding chloride was obtained, from which bichloride of platinum precipitated a beautiful orange-yellow platinum-salt. In a moderately diluted solution which had been previously gently heated, no immediate precipitate was produced; but on cooling, the same salt was deposited in magnificent needles, which could be recrystallized from boiling water, or better from hydrochloric acid. This compound contained

$$C_{16}H_{19}BrPCl, PtCl_2.$$

A difficultly soluble gold-salt, crystallizing from boiling water in small scales, was found to have the corresponding composition,

$$C_{16}H_{19}BrPCl, AuCl_3.$$

Very different results were observed when the whole of the bromine was removed by means of oxide of silver. A powerfully alkaline solution was thus obtained, which, converted into hydrochlorate, gave, with bichloride of platinum, a precipitate only after very considerable evaporation. The precipitate was likewise of a deep orange-red colour; it readily dissolved in boiling water, from which it separated on cooling in the form of well-defined octahedra having the composition

$$C_{16}H_{18}PCl, PtCl_2.$$

Terchloride of gold furnished likewise a crystalline precipitate very similar in appearance to the gold-salt previously mentioned, but containing

$$C_{16}H_{18}PCl, AuCl_3.$$

The action of bibromide of ethylene on triethylphosphine, and the subsequent transformation of the compound produced, is readily explained. The two substances unite in equal equivalents, the product of the reaction being the bromide of a phosphonium, in which the fourth equivalent of hydrogen is replaced by a compound molecule, C_4H_4Br (brominetted ethyle?), of monatomic substitution-power,

$$\text{Bromide of triethyl-bromethylene-phosphonium } \left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ (C_4H_4Br)' \end{matrix}\right\} Br.$$

The compound phosphonium of this bromide possesses very considerable stability, as is sufficiently evinced by its deportment with nitrate of silver, and by the formation of the platinum- and of the

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gold-salt. All my attempts, however, to separate the base itself have entirely failed. Under the influence of oxide of silver, the bromide yields an alkaline solution possessing all the characters of the -onium-bases. The body in solution, however, no longer belongs to the same series, the elements of hydrobromic acid having separated from the original compound metal.

$$\left.\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ (C_4H_4Br)' \end{matrix}\right\} PBr + 2AgO = 2AgBr + \left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} PO, HO.$$

The compound thus obtained may be designated as the hydrated oxide of triethyl-vinyl-phosphonium.

I have ascertained by experiment that the brominetted bromide is by no means the only result of the action of bibromide of ethylene on triethylphosphine, although under favourable circumstances it appears to be the chief product. Invariably a portion of the bibromide, faithful to its traditions, splits into hydrobromic acid and bromide of vinyle; and we find therefore in the white crystalline mass always, together with hydrobromate of triethylphosphine, a certain quantity of the very bromide of triethyl-vinyl-phosphonium, which, as has been stated, results from the action of oxide of silver on the brominetted bromide.

$$2\left[\left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} P\right] + C_4H_4Br_2 = \left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ H \end{matrix}\right\} PBr + \left\{\begin{matrix} C_4H_5 \\ C_4H_5 \\ C_4H_5 \\ C_4H_5 \end{matrix}\right\} PBr.$$

The action of bibromide of ethylene on triethylphosphine, complex as it is, receives an additional element of complication by the influence of heat. Ebullition appears to facilitate the formation of a fourth bromide, which, although less prominently, is also produced in the cold. The study of this compound is not yet completed.

"Action of Bisulphide of Carbon on Triethylphosphine." By A. W. Hofmann, Ph.D., F.R.S.

Among the many characteristic reactions of the phosphorus-bases, their deportment with sulphur is so conspicuous that it has served frequently as a test for the presence of these substances. In continuing the study of the phosphorus-bases, I have found that this remarkable attraction for sulphur is by no means limited to this element in the free state. Many sulphur-compounds, when coming into contact with triethylphosphine, are instantaneously decomposed, their sulphur being appropriated in the formation of the beautiful bisulphide

$$E_3PS_2,$$

which, as has been pointed out on a former occasion, is generated by the action of free sulphur. As an illustration, the deportment of bisulphide of nitrogen may be quoted. This substance, obtained by the action of ammonia on chloride of sulphur, and as yet scarcely touched upon as an agent of research, is instantaneously decomposed

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into its constituents when acted upon by triethylphosphine,

$$\underbrace{E_3P}_{\text{Triethyl-phosphine.}} + \underbrace{NS_2}_{\text{Bisul-phide of nitrogen.}} = \underbrace{E_3PS_2}_{\text{Bisulphide of triethyl-phosphine.}} + N.$$

The reaction is so violent that care must be taken to prevent the phosphorus-base from being inflamed.

Triethylphosphine is not less powerfully attacked by bisulphide of carbon; but the result is different. On mixing the two bodies in the anhydrous condition, they are found to combine with explosive violence, a deep crimson-coloured crystalline compound being produced. This substance is obtained in better crystals if ethereal solutions, instead of the anhydrous compounds, be employed. The new body separates in beautiful crimson leaflets the moment the two solutions are mixed. This phenomenon is so characteristic, that ever since it was first noticed, it has served me as a valuable test for the detection of even minute traces of triethylphosphine. A watch-glass, moistened with the liquid in which the phosphorus-base is suspected, is held over a vessel containing bisulphide of carbon: the vapour of this compound immediately causes the formation of a crimson network of crystals, if the smallest quantity of triethylphosphine be present. It is necessary that the base should be free; its saline solutions are not affected by bisulphide of carbon; the reaction, however, immediately appears when the base is liberated by the addition of an alkali.

The new body produced by the action of bisulphide of carbon upon triethylphosphine is insoluble in water, nearly insoluble in ether, but soluble in alcohol. From boiling alcohol it is deposited on cooling in crimson needles, somewhat similar to the crystals of chromic acid as obtained by the action of sulphuric acid upon bichromate of potassium. The presence of bisulphide of carbon in the alcohol considerably increases its solvent power for the crimson body. The new substance fuses at about 95° C.; it is volatile even at the common temperature, and is easily volatilized at the temperature of boiling water. When rapidly heated it sublimes with partial decomposition.

The crimson crystals appear to have the character of a weak base; they easily dissolve in concentrated hydrochloric acid, a colourless liquid being formed; from this solution potassa or ammonia reprecipitate the body, apparently unchanged, although, in consequence of the finely divided state, of a somewhat lighter colour. The hydrochloric solution gives with bichloride of platinum a bright yellow amorphous salt insoluble in alcohol and ether, which on drying becomes dingy, with indications of decomposition. A gold-salt similarly obtained exhibits a like deportment. Both salts appeared but very little adapted for analysis. The alcoholic solution of the body is decomposed by nitrate of silver with formation of sulphide of silver.

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The analysis of the crimson crystals has shown that they contain

$$C_{14}H_{15}PS_4 = C_{12}H_{15}P + C_2S_4 = E_3P + C_2S_4.$$

They are therefore formed by the direct union of 1 equivalent of triethylphosphine with 2 equivalents of bisulphide of carbon.

In the dry state the bisulphide of carbon compound may be preserved without being altered. In the presence of moisture, however, it is decomposed, especially during hot weather. On examining some specimens which had been kept during several months, the crimson colour was found to have disappeared, the substance had assumed a light yellow colour, and on opening the bottles the odour of sulphuretted hydrogen became at once apparent. The yellowish substance on recrystallization proved to be pure bisulphide of triethylphosphine. I leave it undecided whether this transposition had taken place according to the equation

$$E_3PC_2S_4 + 2HO = E_3PS_2 + 2HS + C_2O_2,$$

or

$$E_3PC_2S_4 + 2HO + 2O = E_3PS_2 + 2HS + C_2O_4.$$

What is the constitution of the crimson body? In mineral chemistry we are acquainted with a compound closely allied in composition and formation to the new compound. Bisulphide of carbon, when treated with an alcoholic solution of ammonia, furnishes, together with other products, a salt crystallized in long lemon-yellow needles, which is known by the name of sulphocarbamate of ammonium.

This compound,

$$(H_4N)H_2N, C_2S_4,$$

when treated with diluted acids, is converted into an oily acid of but little stability, sulphocarbamic acid:

$$HH_2N, C_2S_4.$$

If we replace in this compound the hydrogen by ethyle, the nitrogen by phosphorus, in other words, if we replace the ammonia by triethylphosphine, we arrive at the composition of the body which forms the subject of this note.

I have convinced myself experimentally that trimethylphosphine exhibits with bisulphide of carbon a perfectly similar deportment. The compound formed is likewise of a crimson colour, but of a somewhat lighter tint; it is more volatile and more readily soluble in alcohol than the corresponding ethyle-compound: it is also somewhat soluble in water.

Triethylarsine is not altered by the addition of bisulphide of carbon; after some time, however, long needles are formed in the mixture of the two bodies. These needles are probably an analogous arsenic-compound: I have not however examined them. A mixture of triethylstibine and bisulphide of carbon was preserved for several months, without undergoing any change.

"Contributions towards the History of the Monamines." By A. W. Hofmann, Ph.D., F.R.S.

The progress of my experiments on the poly-ammonias and on the

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phosphorus-bases, now and then involves the study of reactions which are scarcely comprised between the boundary lines of the principal inquiries. For the sake of perspicuity, I beg leave to submit the results of these studies separately to the Society.

1. *Action of Bibromide of Ethylene on Trimethylamine.*

The unexpected result obtained in the action of bibromide of ethylene on triethylphosphine, induced me to examine the deportment of the tertiary amine-bases under the influence of the same agent. As a characteristic representative of this class I have selected trimethylamine, which may be readily procured in tolerable quantity and in a state of purity.

On submitting trimethylamine to the action of bibromide of ethylene, phenomena are observed which are perfectly similar to those which occur in the analogous experiment with triethylphosphine. On account of the volatility of the trimethylamine, I have never worked with the anhydrous base, but invariably either with aqueous or alcoholic solutions. At the common temperature bibromide of ethylene is only gradually acted on by an aqueous solution of trimethylamine. Frequent agitation and contact for several days are necessary to complete the reaction; addition of alcohol accelerates the process; which may be still very considerably shortened by exposure of the mixture in sealed vessels to a temperature of from 40° to 50°. To exclude complication, it is desirable to avoid a higher temperature and to keep always the bromide in considerable excess.

By adopting these precautions, the mixture of the two bodies is soon found to deposit a white crystalline salt, the formation of which continues until the liquid has assumed an acid reaction. A considerable quantity of this salt is dissolved in the water; it is therefore most convenient to distil off the excess of bibromide of ethylene and to evaporate the residuary liquid to dryness. The dry saline mass, separated from a slightly yellowish deliquescent substance by washing with absolute alcohol and once or twice recrystallized from the same solvent, furnishes magnificent white needles, extremely soluble in water, readily soluble in boiling alcohol, much less so in cold alcohol, and insoluble in ether. This salt can be boiled with the fixed alkalies without disengaging a trace of an alkaline vapour. This deportment renders it easy to recognize the absence of impurities.

The composition of this substance, established by many determinations, is represented by the formula

$$C_{10}H_{13}NBr_2 = \left.\begin{matrix} C_2H_3 \\ C_2H_3 \\ C_2H_3 \\ (C_4H_4Br)' \end{matrix}\right\} N, Br.$$

This substance, which presents itself as bromide of trimethyl-bromethylene-ammonium, is formed by the simple union of 1 equivalent of bibromide of ethylene with 1 equivalent of the tertiary mon-

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amine. A glance at the formula exhibits the perfect analogy of the composition of this compound with that of the bromide formed by the action of bibromide of ethylene on triethylphosphine. The deportment of the two salts with nitrate and with oxide of silver is also similar in every respect.

By treatment with nitrate of silver, the bromine not belonging to the ammonium may be removed without affecting the bromine of the radical. The nitrate thus obtained, after separation of the excess of silver, furnishes with bichloride of platinum a difficultly soluble octahedral salt, crystallizable from a large quantity of boiling water, and containing

$$C_{10}H_{13}BrNCl,\ PtCl_2 = \left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\(C_4H_4Br)'\end{matrix}\right\}NCl,\ PtCl_2;$$

and with terchloride of gold an analogous compound crystallizing from boiling water in splendid golden-yellow needles,

$$C_{10}H_{13}BrNCl,\ AuCl_3 = \left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\(C_4H_4Br)'\end{matrix}\right\}NCl,\ AuCl_3.$$

Treatment with oxide of silver converts the bromide of trimethyl-bromethylene-ammonium into the oxide of trimethyl-vinyl-ammonium:

$$\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\(C_4H_4Br)'\end{matrix}\right\}NBr + 2AgO = 2AgBr + \left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NO,\ HO.$$

The solution of this substance is a powerfully alkaline liquid, which, on saturation with hydrobromic acid, furnishes a deliquescent bromide of extreme solubility, entirely differing from the original bromide. The corresponding chloride forms with bichloride of platinum an octahedral salt, likewise extremely soluble in water, but insoluble in alcohol; with terchloride of gold, beautiful yellow needles recrystallizable from boiling water.

$$\text{Platinum-salt } C_{10}H_{12}NCl,\ PtCl_2 = \left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NCl,\ PtCl_2.$$

$$\text{Gold-salt } .. C_{10}H_{12}NCl,\ AuCl_3 = \left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NCl,\ AuCl_3.$$

As might have been expected from the experience gathered in the phosphorus-series, the formation of the brominated bromide is invariably accompanied by the simultaneous production of the vinyl-

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compound, and of a corresponding quantity of hydrobromate of trimethylamine.

$$2\left[\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\end{matrix}\right\}N+C_4H_4Br_2=\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\H\end{matrix}\right\}NBr+\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NBr.$$

Indeed it would appear that at a high temperature and with an excess of trimethylamine, the equation just given represents the principal phase of the reaction. In an experiment made under the stated conditions, the liquid in the digester had assumed a deep yellowish colour; and on evaporation and appropriate treatment a crystalline salt was obtained, which on analysis was found to consist exclusively of

$$C_{10}H_{12}NBr=\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NBr,$$

the mother-liquor containing a large quantity of hydrobromate of trimethylamine. It is possible that even in this reaction the vinyl-compound was only a secondary product, formed by the decomposition of the brominated bromide under the influence of an excess of trimethylamine.

$$\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\(C_4H_4Br)'\end{matrix}\right\}NBr+\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\end{matrix}\right\}N=\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\H\end{matrix}\right\}NBr+\left.\begin{matrix}C_2H_3\\C_2H_3\\C_2H_3\\C_4H_3\end{matrix}\right\}NBr.$$

Exactly as in the phosphorus-series, together with the compounds described, some other substances are formed, particularly when the process is supported by the action of heat. As yet I do not sufficiently understand these additional reactions.

I have established experimentally that triethylamine and triamylamine, when treated with bibromide of ethylene, give rise to similar reactions. I have not, however, minutely examined the substances which are formed. They are sufficiently characterized by theory.

The unexpected deportment of bibromide of ethylene with the tertiary monamines and monophosphines, furnishes a new proof of the fact, that all our rational formulæ are, after all, the expressions of special reactions. With the alkalies, the brominated Dutch liquid behaves as a double salt of two monatomic compounds,

$$(C_4H_3)'Br+HBr.$$

With silver-salts, with aniline, &c., it exhibits the deportment of a true biatomic compound,

$$(C_4H_4)''Br_2.$$

With the tertiary amines and phosphines, lastly, we find that the elements of the same body, in accordance with the requirement of the case, arrange themselves into one monatomic compound, the constitution of which, if we simply consider the function which it

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performs under these special circumstances, might be represented by the formula

$$(C_4H_4Br)'Br.$$

It is obvious that the three formulæ,

$$(C_4H_3)'Br, H Br,$$
$$(C_4H_4)''Br_2 \quad \text{and}$$
$$(C_4H_4Br)'Br,$$

represent the constitution of this body with reference to certain special conditions; the absolute arrangement of the molecules we ignore altogether, and it is doubtful whether it will ever be accessible to experiment.

"An Experimental Inquiry into the alleged Sugar-forming Function of the Liver." By F. W. Pavy, M.D.

The author commenced by stating that the question to be discussed in his communication was, not whether sugar was to be found in the animal system independently of a saccharine alimentation, for that he considered to stand upon irrefutable ground; but whether the sugar encountered in the liver *after death* was a natural representation of the condition during life, or was only the result of a *post mortem* occurrence. He had noticed as early as February 1854, that the blood removed by catheterism of the right ventricle during life, was almost completely destitute of saccharine impregnation. The observation did not then, however, receive the attention it deserved; but on repeating the experiment at a later period, and meeting with a similar result, an investigation was made which has led to the conclusions advanced in his communication.

From upwards of sixty observations, it is asserted that the condition of the blood after death can no longer be taken as indicating its state during life. For, if blood be withdrawn from the right ventricle of the living animal in a natural or tranquil state, there is scarcely an appreciable amount of sugar to be discovered, whilst, if the animal be afterwards sacrificed and blood collected from a fine incision of the ventricle, it will be found to present a strong indication of the presence of sugar. In one of the experiments quoted, there was a barely appreciable reaction in the blood removed during life, and nearly 1 per cent. of sugar in the blood collected after death, the animal having been sacrificed immediately after catheterism has been performed.

Observing this striking contrast in the blood abstracted from the right ventricle *before* and *after* death, the possibility occurred that there might be a corresponding contrast in the organ that was considered to be specially endowed with a sugar-forming function. The recent researches of Bernard had taught us that a material naturally existed in the liver which was extremely susceptible of conversion into sugar. It was this material, in fact, which was looked upon as giving rise to the sugar thought to be largely present in the liver during life. At the outset of the inquiry, an agent was sought for

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which would check the transformation of the sugar-forming material after death, and thus present the liver in a condition as near as possible to that which existed during life. Potash was found to possess this effect without destroying the principles concerned. A strong solution of it was then injected, as instantly after death as practicable, through the portal vein into the liver; and, as the result, the organ presented scarcely any appreciable trace of the presence of sugar. A liver similarly treated when it had been allowed to remain a short period after death, gave the usual strong reaction of sugar that has been hitherto noticed. By injecting only a part of the organ with the alkali, it is most strikingly susceptible of demonstration, that the presence of sugar is in reality due to a *post mortem* occurrence, and can therefore be no longer looked upon as a representation of the natural *ante mortem* condition.

The sudden abstraction of heat from the liver instantly after death, leads to a similar arrest of the production of sugar, and thus enables us likewise to represent the real condition of the organ belonging to life. In one of the experiments mentioned, where a dog was sacrificed, and a piece of the liver instantly sliced off and thrown into a freezing mixture of ice and salt, the absence of sugar was almost complete; the amount at least was so small, that it was found impossible to arrive at a quantitative determination with a concentrated spirituous extract, notwithstanding the process is susceptible of so great a delicacy. The portion of the liver which was not submitted to the action of cold, and which was allowed to remain a short time in the animal, yielded on analysis an indication of 2·96 per cent. of sugar.

Division of the spinal cord in the lower part of the cervical region, the effects of which have been noticed by Bernard, but differently interpreted, leads to a corroboration of the deductions drawn from the preceding experiments. When the weather is cold or moderate, the operation is followed by a gradual reduction of temperature; and if the animal be sacrificed when its body has cooled down to about 70°, the liver is found free from sugar, upon an ordinary immediate examination, because at such a degree the *post mortem* transformation is not effected with sufficient rapidity to lead to our deception. Placed aside, however, it soon becomes strongly saccharine. Should the operation of division of the cord be performed, and the temperature of the animal be afterwards maintained at about the ordinary height by exposure to external warmth, then the liver is as strongly saccharine upon ordinary examination after death, as if the animal had been taken and simply sacrificed.

By oiling the coats of rabbits and exposing them to cold, the temperature of the body falls, and precisely the same phenomena are noticed as after division of the cord.

With frogs in a vigorous condition, the presence or absence of sugar in the liver submitted to the ordinary process of examination after death, is dependent upon the temperature of the animal at the time of the destruction of life. This fact was independently noticed by myself about the time that it was mentioned by Bernard in a

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communication to the Parisian Academy of Sciences. Bernard's interpretation of it is connected with the relative activity of the abdominal circulation; but, for myself, I bring it forward as strongly supporting the views that have been advanced, and consider it to be explained by the influence of temperature on the *post mortem* production of sugar.

The material which occasions the presence of sugar in the dead liver, has been called by Bernard "Glucogenic matter,"—a term which, being only specially applicable after death, it is suggested should be abandoned, and replaced by Hepatine.

The amount of hepatine in the liver of the dog is much greater under a vegetable than an animal diet. The amount is also increased by mixing sugar with animal food. From the examples given, it is shown likewise that the relative weight of the liver presents a proportionate variation, according to the quantity of hepatine present. In eleven dogs taken indiscriminately, that had been restricted to an animal diet, the weight of the liver was one-thirtieth that of the animal. The average per-centage of hepatine yielded by eight livers, also taken indiscriminately after an animal diet, was 6·97. Five instances have been collected of dogs restricted to a vegetable diet for some days prior to death. The average weight of the liver was one-fifteenth that of the animal. In only three of the examples was the actual amount of hepatine determined, but in the other two it was noticed to be exceedingly large. The average given by the three was 17·23 per cent. Four dogs were placed upon an animal diet, and about a quarter of a pound of ordinary cane-sugar administered daily for a short period. The average weight given by the four livers was one-sixteenth and a half that of the animal, and the average amount of hepatine yielded was 14·5 per cent.

The natural destination of hepatine in the living body remains to be determined. It has also to be shown how it resists transformation into sugar during life, when it is so rapidly changed at an elevated temperature immediately after death. A possible analogy may be presented by the following occurrence:—When a solution of hepatine, *in a neutral state*, is placed in contact with saliva, an almost instantaneous transformation into sugar takes place; but if a little acid alkali or carbonated alkali be added, scarcely a trace of change is for some time discoverable.

Under normal circumstances, rarely an appreciable amount of sugar is encountered in the circulatory system—only, according to my analyses, from about ·047 to ·073 of a grain in 100 grains of defibrinated right-ventricular blood; and this would appear to result rather from a simple escape of a small amount of hepatine from the tissue of the liver into the blood whilst circulating through the capillaries, than from a special functional operation of the organ; for when a disturbance of the circulation, whether by congestion or the opposite, is occasioned, sugar makes its appearance to a considerable extent in the system, because the admixture of hepatine with the blood is favoured. It can be easily shown by experiment, that on introducing hepatine into the circulatory system, a saccharine state

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of the blood is induced, and if enough have been employed, a strongly marked diabetic condition of urine is established.

Sacrificing an animal and maintaining the circulation by performing artificial respiration, occasions a well-marked diabetes. With the destruction of life, the transformation of hepatine into sugar takes place, and this, being carried away by the blood, is eliminated by the kidneys, and thus renders the urine strongly saccharine.

Many phenomena which were before obscurely explained, receive a lucid interpretation from the new facts which have now been brought to light.

"On the Action of Bile upon Fats; with Additional Observations on Excretine." By W. Marcet, M.D., F.R.S.

Having formerly observed and communicated to the Société de Biologie of Paris, that by heating a solution of neutral tribasic phosphate of soda ($2NaO . HO . PO_5$) mixed with animal fatty acids, an emulsion was obtained attended with the formation of a small quantity of soap, while no such action occurred if neutral fats were used instead of fatty acids, I was induced to inquire into the nature of the action of bile on neutral fats and fatty acids (sheep's bile being used), with the final object of throwing, if possible, some additional light on the digestion of fats. These investigations led to the following results:—

1. A mixture of bile and neutral fats (stearine, oleine and margarine), heated to a temperature above the fusing-point of the fat, undergoes no change, and no chemical action takes place.

2. A mixture of bile and fatty acids (stearic, oleic, and margaric acids), heated to a temperature above the fusing-point of the fatty acids, is transformed into a solution, a very few and minute globules only of fat remaining unacted upon from the presence of oleic acid. This solution becomes a perfect emulsion on cooling, and is attended with a chemical decomposition of the bile; and further, if the emulsion of bile and fatty acids be filtered when quite cold, and the residue on the filter thoroughly washed with distilled water, the filtrate and washings mixed together again possess the property of forming an emulsion with another quantity of fatty acids, being also at the same time partly decomposed, although in the previous operation the bile appeared to have exhausted its power on the fatty acids. The filtrate and washings from this second operation again act upon a fresh quantity of fatty acids, and so on; only in every subsequent operation the proportion of emulsion obtained appears to diminish, and the induced chemical decomposition to be lessened.

3. Pure oleic acid, when agitated with bile, cold or hot, produces no emulsion or chemical action whatever.

4. The stomach during digestion has the power of decomposing the fats contained in the food into fatty acids, fats acquiring thereby the property of being acted upon chemically by the bile, and of being transformed into an emulsion.

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The chemical action, or saponification, induced by the fatty acids under the above circumstances, was proved by the mixture acquiring a strong acid reaction; and it was further observed that the acid filtrate from the cold emulsion was not precipitated by hydrochloric acid, showing apparently that fatty acids exert on bile a chemical decomposition at least as extensive as hydrochloric acid. With the view of determining precisely the amount of soap formed, a method of analysis was adopted calculated to indicate the proportion of fatty acid remaining unacted upon by the bile: the difference between the fatty acids used and the result of the above operation was equal to the weight of the fatty acids saponified. It was found, in three analyses, that the mixture of bile and fatty acids being exposed for three hours (in Analysis II. for 3½ hours) to the heat of an open water-bath, contained an amount of soap in which the proportion of fatty acids was 30·21 per cent., 20·5 per cent., 11·5 per cent. of that employed in the analysis. The filtrate from the emulsion in analysis No. II., mixed with the solution obtained by washing the emulsion with distilled water, was treated for three hours on the water-bath with a fresh quantity of fatty acids, which operation yielded a proportion of fatty acid saponified equal to 12·7 per cent. of that used in the analysis. Finally, the filtrate and washings obtained in this last group were mixed with another quantity of fatty acids, and exposed for three hours to the heat of the water-bath, in which case the proportion of fatty acid saponified was equal to 3·8 per cent. of that used in the analysis. The various operations had been attended with the formation of an emulsion.

In order to be certain that, after exposing a mixture of bile and fatty acids to the heat of a water-bath for three hours, the chemical action thus induced was completely exhausted, two analyses were undertaken according to the process just mentioned, and with bile from the same gall-bladder; but in one operation the mixture was heated for three hours, and in the other for six hours: the proportion of fatty acid saponified was the same in both cases, showing that after three hours the bile had ceased to act on the fatty acids.

Having obtained the above results, an inquiry was next undertaken respecting the state of the fats of food in the stomach during digestion. For this purpose the contents of the stomach of several dogs, fed with cooked meat and neutral sheep's fat, were examined at different stages of digestion; the acids of the stomach soluble in water were removed by protracted washings with distilled water, and the residue being treated with alcohol and ether, yielded solutions found to contain fatty acids. In some cases the contents of the stomach were first treated with alcohol, and the fatty matters thus obtained subsequently washed with distilled water, and finally again dissolved in alcohol and ether. These analyses constantly yielded fatty acids, which, when heated with fresh sheep's bile, were found to dissolve and produce an emulsion.

In order to determine whether the cooking of the meat with which

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the dogs had been fed had transformed any of the neutral fats into fatty acids, a sample of roast meat was mixed and washed with distilled water until the washings had completely lost their acid reaction; the meat was then mixed with alcohol and allowed to stand for more than a week. After that time the fluid was found to be perfectly neutral, showing that no fatty acids had been formed.

From these researches it appears that the presence of bile in the intestines is closely connected with the digestion of fats.

The results of recent investigations on excretine show that this substance exists on an average in the proportion of 0·460 grm. for one evacuation when the excretine is impure, and of 0·184 grm. when it is pure. From the careful examination of the fæces of a child one year old, I have ascertained that they invariably contained no excretine, but cholesterine; the proportion of the latter, purified by repeated crystallizations, being equal to 0·036 grm. in one evacuation, which number is, however, a very low estimate. Nothing in the food could account for this singular result. It is therefore most probable that excretine is only present in the evacuations of the full-grown or adult individual.

I have been most ably aided in these investigations by my assistant, Mr. Frederick Dupré, Ph.D.

GEOLOGICAL SOCIETY.

[Continued from p. 77.]

January 5, 1859.—Prof. J. Phillips, President, in the Chair.

The following communications were read:—

1. "On Fossil Plants from the Devonian Rocks of Gaspé, Canada." By Dr. J. W. Dawson, F.G.S., Principal of McGill's College, Montreal.

The plant-bearing rocks in the peninsula of Gaspé were first noticed by Sir W. E. Logan in 1843. To determine these fossil plants accurately, it was necessary to study them in place. With this view Dr. Dawson visited Gaspé last summer, and carefully examined the localities by the aid of the plans and sections of the Geological Survey of Canada. The strata referred to have a vertical thickness of 7000 feet, as estimated by Sir W. Logan; they rest on Upper Silurian rocks, and underlie the Carboniferous conglomerates; and some beds contain Lower Devonian Brachiopods, &c.

Among the vegetable remains determined by Dr. Dawson is a curious genus, termed by him *Psilophyton*, which belonged to the *Lycopodiaceæ*, and had minute adpressed leaves on slender dichotomously-branching stems, with circinate vernation, and springing from a horizontal rhizome, which had circular areoles with cylindrical rootlets. Some of the shales are matted with these rhizomes. Obscure traces of fructification are observable in cuneate clusters of bracts. The fragments of the different parts of this in-

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teresting plant might easily be mistaken for portions of other and very distinct plants, such as *Karstenia*, *Halonia*, *Stigmaria*, *Schizopteris*, *Trichomanites*, Fucoids, &c. The author describes two species of *Psilophyton*, *P. princeps* and *P. robustius*.

Dr. Dawson further described a new form of *Lepidodendron* (*L. Gaspianum*); also some specimens of Coniferous wood related to the *Taxus* (*Prototaxites Logani*), and some less clear forms belonging to *Knorria*, *Poacites*, &c. The author also noticed the occurrence of *Entomostraca* (*Beyrichia*), *Spirorbis*, occasional fish-remains, some Brachiopods, and also rain-marks and ripple-marks in these Devonian beds.

2. "On some Points in Chemical Geology." By T. Sterry Hunt, Esq., of the Geological Commission of Canada. (Communicated by Prof. A. C. Ramsay, F.G.S.)

§ I. Referring to his communications to other Societies in which he had endeavoured to explain the theory of the transformation of sedimentary deposits into crystalline rocks, and to the researches of Daubrée, Senarmont, and others, the author remarked, in the first place, that the problem of the generation, from the sands, clays, and earthy carbonates of sedimentary deposits, of the various siliceous minerals which make up the crystalline rocks, may be now regarded as solved; and that we find the agent of the process to be water, holding in solution alkaline carbonates and silicates, acting upon the heated strata. Under some circumstances, however—such as the presence of gypsum or magnesia—such anomalies might occur as are presented by the comparatively unaltered condition of some portions of the strata in metamorphic regions.

§ II. Many crystalline rocks, formerly regarded as of plutonic origin, are now found to be represented among altered sedimentary strata; and the chemical student in geology is now brought to the conclusion that metamorphic rocks, such as granite, diorite, dolomite, serpentine, and limestone, may, under certain conditions, appear as intrusive rocks. This is chiefly owing to the pasty or semi-fluid state which these rocks must have assumed at the time of their displacement.

§ III. The author next remarked that the hypotheses relating to the origin of the two great groups of plutonic rocks—those with potash and much silica, and those with soda and less silica—are not satisfactory.

§ IV. Mr. Hunt, considering that the water of the early palæozoic ocean differed from that of the modern seas, in that it contained chlorides of calcium and magnesium to a far greater extent, especially the former, sulphates being present only in small amount, noticed that the replacement of the chloride of calcium by common salt involved the intervention of carbonate of soda and the formation of carbonate of lime; and that the continual decomposition of alkaliferous silicates to form the vast masses of argillaceous sediments from the felspathic minerals of the earth's crust, must have formed, and is

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still forming, alkaline carbonates which play a most important part in the chemistry of the seas.

§ V. The study of the chemistry of mineral waters, in connexion with that of sedimentary rocks, leads the author to believe that the result of processes continually going on in nature is to divide the silico-argillaceous rocks into two great classes; the one characterized by an excess of silica, by the predominance of potash, and by the small amounts of lime, magnesia, and soda, and represented by the granites and trachytes; while in the other class silica and potash are less abundant, and soda, lime, and magnesia prevail, giving rise (by metamorphism) to triclinic felspars and pyroxenes. The metamorphism and displacement of sediments may thus, he observed, enable us to explain the origin of the different varieties of plutonic rocks without calling to our aid the ejections of a central fire. (See § III.)

§ VI. The most ancient sediments, like those of modern times, were doubtless composed of sands, clays, and limestones; but, on the principles laid down in §§ IV. and V., the author shows that the chemical composition of the sediments in different geologic periods must have been gradually changing. Illustrating his views by the condition of the Canadian rocks, Mr. Hunt observes that, on the large scale, in the more recent crystalline or metamorphic rocks, we find a less extensive development of soda-felspar, while orthoclase and mica, chlorite and epidote, and silicates of alumina, like chiastolite, kyanite, and staurotide (which contain but little or no alkali, and are rare in the older rocks), become abundant. The decomposition, too, of the rocks is more slow now, because soda-silicates are less abundant, and because the proportion of carbonic acid in the air (an efficient agent in these changes) has been diminished by the formation of limestones and coal.

§ VII. The author accepts the views of Babbage and Herschel as to the internal heat of the earth rising through the stratified deposits, on account of the superficial accumulation of sediments, metamorphosing the rocks submitted to its action, causing earthquakes and volcanic irruptions by the evolution of gases and vapours from chemical reactions, and giving rise to disturbances of equilibrium over wide areas of elevation and subsidence.

§ VIII. Mr. Hunt observes that the structure of mountain-chains, both those due to the uprise of metamorphosed rocks through tertiary and secondary deposits, and those formed of older masses of sediment, contorted and altered, bears out the principles of § VII.

January 19.—Prof. J. Phillips, President, in the Chair.

The following communications were read:—

1. On the Gold-field of Ballaarat, Victoria. By H. Rosales, Esq. Communicated by W. W. Smyth, Esq., Sec. G.S.

Mr. Rosales described the position of the quartz-lodes (the matrix of the gold) in the schists of the hill-ranges, from whence originate the numerous auriferous gullies, forming eventually several channels

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(charriages), and the different courses of the old gold-bearing streams, which gradually passing to lower levels, reach the great areas of basalt, under which they continue their hidden course. To illustrate these points, the author prepared and sent a MS. map of the district from beyond Buninyong to Creswick, on which the granite, basalt, schists, and quartz-lodes were shown, as well as the gold-channels, gullies, runs, leads, &c., connected with which 96 named spots or diggings were carefully indicated.

2. Description of a New Species of Cephalaspis (*C. Asterolepis*) from the Old Red Sandstone of the neighbourhood of Ludlow. By John Harley, Esq. Communicated by Prof. Huxley, F.G.S.

This new form of *Cephalaspis* (from Hopton Gate) is at least twice the size of *C. Lyellii*, and is further characterized by the position, obliquity, and magnitude of the orbits. The space between the orbits is proportionally small, and the occipital crest very short. The outer enamel-layer is ornamented with tubercles, which, though somewhat variable, bear so close a resemblance to those covering the bony plates of *Asterolepis*, as to have suggested the specific name. The inner layer of the bony plate presents lacunæ and canaliculi, resembling those of human bone; and many of them, in the specimen described, are naturally injected with a transparent blood-red material, so distinctly and delicately, that in their minutest details the structure of canals not more than $\frac{1}{50000}$th of an inch in diameter is beautifully revealed.

Mr. Harley also described a more perfect specimen of *Cephalaspis Salweyi* than the one on which Sir P. Egerton not long since determined the species. It was found by Mr. Salwey at Hinstone near Bromyard. Associated with the *C. Salweyi*, the author found a specimen of either a dermal plate or a tooth of a placoid fish, resembling some Silurian fossils called *Cœlolepidæ* by Pander.

XXII. *Intelligence and Miscellaneous Articles.*

NEW METHOD OF EXAMINING AND VERIFYING THE SPECIFIC GRAVITY OF BODIES. BY M. A. MEYER.

THE methods at present in use for the determination of specific gravities are very exact, but at the same time very complicated. As the whole question consists in facilitating the means of measuring exactly the volume of water equivalent to the volume of the substance experimented upon, the problem may be solved in a very simple, but at the same time sufficiently exact manner, by operating as follows:—After having filled a vessel with water, the long leg of a reversed siphon is inserted; the liquid runs out for a moment, but comes to a stand in the tube if the apparatus remains tranquil. The body of which the specific gravity is to be determined is then immersed, and the water recommences to run out by the siphon. Collected in a small receiver, this water represents the exact volume of the body.

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The author states that he has determined by this method a large number of specific gravities which had been determined by older methods, and had found complete agreement. He thinks that this process might be applied with advantage to the determination of the specific gravities of minerals, and in general of bodies whose volume prevents their being submitted to the hydrostatic balance.—*Comptes Rendus*, December 20, 1858.

NOTE ON A THEOREM IN SPHERICAL TRIGONOMETRY.

BY A. CAYLEY.

I am not aware that the following theorem has been noticed: viz., in any spherical triangle, if as usual a, b, c are the sides, and A, B, C the opposite angles, then

$$\sin b \sin c + \cos b \cos c \cos A = \sin B \sin C - \cos B \cos C \cos a,$$

$$\sin c \sin a + \cos c \cos a \cos B = \sin C \sin A - \cos C \cos A \cos b,$$

$$\sin a \sin b + \cos a \cos b \cos C = \sin A \sin B - \cos A \cos B \cos c.$$

The demonstration is very simple; in fact we have

$$\begin{aligned}
&\sin b \sin c + \cos b \cos c \cos A \\
&\quad = \sin b \sin c (\sin^2 A + \cos^2 A) + \cos b \cos c \cos A \\
&\quad = \sin b \sin c \sin^2 A + \cos A (\cos b \cos c + \sin b \sin c \cos A) \\
&\quad = \sin B \sin C \sin^2 a + \cos A \cos a \\
&\quad = \sin B \sin C (1 - \cos^2 a) + \cos A \cos a \\
&\quad = \sin B \sin C + \cos a (\cos A - \sin B \sin C \cos a) \\
&\quad = \sin B \sin C - \cos B \cos C \cos a,
\end{aligned}$$

which proves the theorem.

2 Stone Buildings, W.C.,
January 5, 1859.

DESCRIPTION OF THE METHODS USED TO ASCERTAIN THE FIGURE OF OPTICAL SURFACES. BY M. L. FOUCAULT.

In this note the author describes three processes which he uses conjointly to examine the surface of glass mirrors, in order to discover those parts at which the local corrections must be made, and which he applies afterwards.

The first consists in placing in one of the conjugate foci of the surface a luminous point, in order to observe with the microscope the condition of the pencil reflected about the point of convergence; it is then seen to be decomposed into partial images, the consideration of which furnishes sure indications as to the condition of the surface itself.

The second method is founded on the employment of an object with parallel sides, such as a small piece of steel wire, which is placed in one of the conjugate foci, and whose image is observed at

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a distance by means of a little telescope of small magnifying power, provided with a diaphragm corresponding in extent to the pupil of the human eye. Under these conditions the apparent image is formed in its different parts by the different elements of the mirror; and if these elements have not a common focus, an image of the distortions results, which, suitably interpreted, lead to the discovery of the faults of the rays of curvature corresponding to the different parts of the mirror.

The third process shows directly, by a comprehensive view, the alterations of form reflected on the figure which the mirror ought to present under the circumstances in which the trial is made. The mirror is so arranged as to give in space the image of a narrow orifice pierced in an opake plate, and vividly illuminated by artificial light. This image is almost totally masked by an opake screen with rectangular edge. The rays, which in passing graze the edge, are received directly on the eye, and give an image of the surface of the mirror which is perceived in light and shade, and in which all the reflexions capable of altering the exact convergence of the entire pencil are presented in exaggerated relief. From this the parts are discovered where the corrections ought to be brought, which is accordingly done.

The same methods of examination may also be applied to the achromatic objectives of telescopes, and allow the application of the same system of local corrections.—*Comptes Rendus*, Dec. 20, 1858.

ON SOME LARGE SOLAR SPOTS. BY W. R. DAWES.

Sir,—There is now visible an enormous mass of spots on the sun's disc, composed of two distinct groups near together, each of which has a large spot with many smaller ones near it. Without telescopic aid a sharp eye, properly defended with a dark glass of good colour, will readily see them as two small black spots, very close together, a little below the centre and to the right hand of it. With a powerful telescope they are wonderful and instructive. The eastern large spot is especially so, as exhibiting very distinctly the dark, mottled, cloudy stratum, with the black opening, constituting the true nucleus, nearly in the middle of it.

It is much to be regretted that the photographic art has not yet succeeded in exhibiting any of the details of these interesting phænomena; and to depict them correctly with the pencil is most laborious, if not absolutely impossible. Moreover, the changes in the minuter details are often so great and so rapid, that, if perfectly correct pictures could be obtained from day to day, they would agree only in their general features. The successful application of photography to this department of astronomical observation is surely worthy of all the ingenuity which could be brought to bear upon it. —*Times*, Jan. 28, 1859.

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THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1859.

XXIII. *Lunar Influence on Temperature as connected with Serenity of the Sky.* By J. PARK HARRISON, *Esq.**

[With a Plate.]

1. A RECURRENCE of phænomena affecting the temperature of the air at corresponding periods of the moon's age having led me to believe that lunar influence was susceptible of a higher degree of proof than had hitherto been attained, tables and curves of mean temperature were constructed for a series of years at Dublin and Greenwich (viz. for the years 1836–46 at Dublin, and 1846–56 at Greenwich), and a careful examination was instituted into the average mean temperatures of the lunation, with results that appeared to be well established for the term of years over which the observations extended.

It was found that the mean temperature between new moon and first quarter, and at last quarter, was in each case considerably lower than the mean temperature at new moon, first quarter, and shortly before last quarter: the third day before first quarter, and the second day after last quarter appearing on the annual mean to be the days of minimum mean temperature; and the second day after first quarter the day of maximum mean temperature†.

Further evidence of lunar influence was then traced in the frequent and often alternate recurrence of maximum and minimum mean temperatures for the month on the same days of the moon's age, or at lesser lunar intervals, during successive lunations, not

* Communicated by the Author.

† In an earlier series of years the maximum is found to occur on the day of the first quarter.

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only at Dublin and Greenwich, but at the Cape, Madras, and Toronto.

And lastly, maximum or minimum mean temperatures were found to prevail on certain days of the lunation more than on others,—for example, more maxima than minima on the third day before new moon, and on the second day after first quarter, and more minima than maxima on the second day after third quarter, both at Dublin and Greenwich, for a series of years embracing more than 400 lunations*.

These facts were communicated to the British Association in the years 1857 and 1858, with the object of bringing them as early as possible before the notice of meteorologists; and they have since been confirmed by Mr. J. C. Bloxam, from an examination of observations extending over sixteen years in the Isle of Wight. Thus in the years 1841–56 the temperature at Newport was found to be 0°·8 lower at the third or last quarter than at the first: and during five days at first quarter (from the third day before to the second day after that phase), there was an increment of temperature amounting to 0°·226 per diem; the corresponding days at last quarter showing a decrement of temperature amounting to 0°·110 per diem. The values for N. and S. declination in the summer and winter months were also found to point to the existence of lunar influence in a very marked manner†. I may add that it has now been ascertained that at Greenwich the mean temperature of the second day after last quarter is, on the average of forty-three years, 0°·9 below that of the second day after first quarter.

2. Before proceeding with the subject to which I wish to draw attention in the present communication, it will be necessary to explain that the curve of mean temperature for ten years at Greenwich alluded to in paragraph 1, has been remodelled. It is now formed from the observations of mean temperature for the ten years commencing November 1845 and ending October 1855 (Plate I. fig. 2); and the mean temperature of the ten preceding years, commencing November 1835 and ending October 1845, has been formed into a second curve at the same station (fig. 3). The temperatures of the two decades presented a remarkable contrast. From the means of each year from 1845 to 1855, as given by Mr. Glaisher in the Philosophical Transactions, it was found that the mean temperature of the years 1845–55 was 49°·4; of the years

* The ratio in which maxima occurred at Greenwich from 1814 to 1835, and at Dublin from 1830 to 1852, was as follows:—On the third day before new moon, as 3 : 2; on the second day after first quarter, 2 : 1. The ratio which minima bore to maxima on the second day after third (or last quarter) was 3 : 1.

† Meteorology of Newport in the Isle of Wight, p. 143.

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1835–45, 48°·1*. Now on referring to the curves (figs. 2 and 3), it will be seen that, notwithstanding this great difference, the effects which had been previously remarked in the Dublin and Greenwich curves are still in each instance very apparent. That is to say, the mean temperature is above the average in both curves, (1) at the period of new moon, (2) at first quarter, and (3) before last quarter. It is below the average, (1) between new moon and first quarter, (2) before and after full moon, and (3) at and after last quarter.

These six periods of high and low temperature should therefore be conspicuous in the curve of twenty years' mean temperature. And this is also found to be the case; more particularly at new moon, and for five or six days before and after it.

3. It was early perceived, however, and would seem to be a point of much importance, that a peculiar influence, which appears to be exerted about the middle of the lunation, advances or retards effects which nevertheless still show themselves plainly a day or so earlier or later in the curves. The rise in temperature at full moon (see *a* in Plate), which so often takes place for a short time in what is otherwise a period of low mean temperature, is an instance of this. On referring to the curves it will be seen that, in the instance when the rise occurred *before* full moon, the mean temperature is highest before first quarter; when it was *after* full moon, the rise occurs after first quarter. This movement may possibly prove to be a law rather than a disturbance; but whether this turn out so or no, it is evident that the action would in a series of years obliterate very regular and systematic lines in the curves. For this reason, and also because different effects attend upon lunar influence at different seasons of the year, it appeared to be the better plan to form curves of mean temperature for individual months. The remarkable manner in which the low temperature in November last, whilst defying the expectations of meteorologists, followed the outline of the curves of annual mean temperature at Greenwich, hastened the commencement of the scheme.

4. The long-continued frost of that month, it will be remembered, set in about the 9th day of November, or the fourth day after new moon. It was interrupted by a thaw of three days' continuance at first quarter, which raised the mean temperature to 42°; after which the cold set in with renewed intensity, till on the 19th day of the month—two days before full moon—it was 13 degrees below the average of that day (of the month)

* The average mean temperature of the curves is somewhat higher in both instances. This is due to commencing the meteorological year with November, and to the omission of the means of the observations in the octant columns.

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for forty-five years. On the 20th day there was a slight rise, reducing the deficiency to 10½ degrees, which was still further reduced on the 21st (the day of full moon) to 8 degrees deficiency. On the three following days the temperature fell again, reaching its minimum on the 24th; and then, on the 25th day—two days before last quarter—the frost broke up. These variations in temperature were found to follow the line of the lunar curves of annual mean temperature, even in the rise which, as it has been stated, occurs so frequently at the time of full moon. It should be mentioned that the wind continued steadily in the north or north-east during the short thaw at first quarter, and the rise at full moon.

5. So striking was the aspect of the month, that it led Mr. Glaisher (from whose description of the weather in the daily papers the above figures are abstracted) to search for similar periods of continued cold in former Novembers. During the last forty-five years, it appeared from the investigation that he then instituted, there had been no period analogous to it since the year 1815. There were, however, eight periods which were noticeable; and on referring to the Nautical Almanac, I found that they occurred on the following days of the lunation:—viz., in 1816, between new moon and first quarter; in 1827, also between new moon and first quarter; in 1829, from the day before last quarter to the day before new moon; in 1838, from the same day as the last to the third day after last quarter; in 1841, between new moon and first quarter; in 1849, from the third day after first quarter to the day before full moon; in 1851, from the day before last quarter to the third day after last quarter; in 1856, between new moon and first quarter,—in all the above instances at the three periods of supposed depression in the mean temperature of the lunation.

6. A table was now formed of the mean temperature of each day of the moon's age at Greenwich for forty consecutive Novembers; in the following manner:—The mean temperature of the days on which the moon entered upon her four principal phases being first set down in columns arranged vertically at equal distances, at whatever hour of the civil day the changes may have occurred, the remaining observations were entered in intermediate columns. Thus, if the day of the new moon fell on the fourth day of the month, the mean temperature of that day having been first set down, the means of the third and fifth days of the month were entered in the columns immediately adjoining on either side; and the observations in these columns would then be considered as the mean temperatures of the first day before and the first day after the day of new moon*. And so

* It is hardly necessary to point out that the means of the observations

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with the remaining mean temperatures. In consequence, however, of the ever-varying position of the moon in her orbit, and the greater or less speed with which she happens to be travelling, there continually occur an unequal number of observations between the quarters, equally distributed in a long series of years over the whole lunation. For example, in the case of the first lunation in the present year (1859), new moon falling on the 4th day of January at $5^h\ 25^m$ A.M., the first quarter occurred on the 12th day at $7^h\ 22^m$ A.M., and full moon at $11^h\ 48^m$ PM. on the 18th day. It follows that there would be seven observations of mean temperature between new moon and first quarter, and five only between first quarter and full moon. In the latter case it is presumed that the deficiency would be not improperly made good by repeating the observations of mean temperature of the third day after first quarter, or the third day before full moon, so as to complete the full number of observations in all excepting the octant columns. The means of these columns were not used in forming the curve of November temperature, or the curves of yearly mean temperature, which were derived from tables constructed in a similar manner to the one above described.

In the curve of M. T. for forty Novembers (fig. 5), which was formed from this table, it will hardly be necessary for me to draw attention to the remarkable alternation of high and low temperature which prevails at regular intervals through the lunation. Contrasted with the two curves of ten years' M. T., the first half follows the outline of the corresponding half of the curve fig. 2. The other half, from the day before full moon, very nearly resembles the corresponding period in the curve fig. 3. The principal difference consists in the excess of effects produced; and I cannot doubt that the more pronounced character of the November curve, as compared with the line of the 10-year means, is due to the absence of the counteracting influences in other months, and of the action and reaction which occurs in successive lunations, though the numerical difference of the observations from which the means were deduced must also be taken into account.

7. That very sharp frosts occur on days when the sky is clear requires no demonstrative proof; and so in summer nights the disappearance of clouds, or prevalence of blue sky, at the period of full moon, which appears to be established as a fact on the evidence of Humboldt, Sir John Herschel, and other astronomers of eminence, might, it was seen, have afforded a physical explanation of the depression in the curve at the

in each column represent the mean temperature of more than a single day of the lunation.

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time of opposition, had there been ground for supposing that a similar cloud-dispelling power was prevalent, though it had been overlooked, at other periods of the lunation. The link that was wanting to connect the two phænomena seemed to be supplied by results obtained by Mr. M. J. Johnson, the Radcliffe Observer at Oxford, who, I found, had not only noticed that clouds disappeared at other times besides full moon, but had made special observations connected with the subject in the years 1844, 1845, and 1846. During this period the action was found to commence about the fourth or fifth day of the moon's age, and recurred with intervals up to the fourth or fifth day before the conjunction*,—thus marking at both extremities of the lunation the very points at which minimum mean temperature had been observed, and rendering it a matter of the highest probability that the depressions in the curves of mean temperature were connected with the greater serenity of the sky, and that the periods of high mean temperature would also be found to depend, on an average of years, in some measure upon the amount of cloud. Other circumstances which had come under my own notice, or had been collected from various sources, seemed to point to the same conclusion†.

8. Supposing, then, that it were proved that the two phænomena were connected, it is evident that the effects which have been observed might be attributed to one of two causes, or to both. High mean temperature, for example, might be due to heat extricated upon the condensation of vapour into cloud and rain—or to the law of radiation already referred to, by which in certain conditions of the atmosphere heat is retained in the lower strata of the air, more especially when the sky is entirely covered with thick clouds. It is well known that opposite effects occur during a clear state of the atmosphere, even in summer lunations, when the mean temperature of the hottest day will often be reduced by the action of terrestrial radiation at night to a degree that could *à priori* have hardly been conceived possible.

9. The two-hourly observations of the amount of cloud which were taken at Greenwich, day and night, from 1840 to 1847, provided the only means of testing the fact of the dispersion or absence of clouds at different periods of the moon's age which were attainable. Valuable, however, as these observations were for ordinary meteorological purposes, they were not so well

* In other years the cloud-dispelling power is found to be exerted earlier in the lunation—seldom *later* than the day mentioned in the text. At and before new moon the sky is often concealed by a thin veil of cloud, much as on the day of the eclipse in March 1858.

† Mr. Nasmyth also has noticed the phænomenon of the dispersion of clouds about the fourth day of the moon's age.

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adapted to the present inquiry. In addition to the blanks caused by the cessation of observations on the Sunday, which could not, in an investigation into lunar influence, be supplied by means subjected to ordinary modes of correction, there were errors to be allowed for which appeared inseparable from the measurement of rapidly-passing and ever-changing bodies of vapour. No distinction, also, could be made in the daily means between the more dense and rarefied clouds. Notwithstanding this, the evidence derived from the above observations is most important. Fig. 1 is a curve formed from tables of the mean daily amount of cloud for seven years at Greenwich, arranged in the same manner as in the tables of mean temperature, which have been already described, excepting that the sums of each vertical column were divided by the number of observations actually recorded: they varied from 70 to 76. This cloud-curve will be found to agree in a remarkable manner with the curve of M. T. for ten years at Greenwich (fig. 2), with which it accords most nearly in point of time.

10. Struck by the apparent regularity and boldness of the lines of November temperature, I then formed a curve of the mean amount of November cloud for the years 1841–48 at Greenwich. It was formed in the same manner as the 7-year curve, with this exception—that the arithmetic means of the days immediately before and after the blank days in the tables were from necessity introduced to make up an equal number of seven observations in each column throughout the lunation. It was found that the mean of the means of these columns was the same as the mean amount of cloud for November, as given in the results of the Greenwich observations, viz. 7·3. The curve (fig. 6) which was then formed proved a perfect reflexion of the mean temperature of November for forty years, as obtained from the lunar tables.

11. The results of the Greenwich observations for 1840–47 supplied a further test, or rather index of the serenity of the sky at different periods in the lunation, and so of the effects on temperature produced by an absence of cloud. On extracting the cloudless days, or what might be considered cloudless days, there enumerated, and arranging them in their proper position on the lunar curves of mean temperature, in the three first years of the above-named period, out of twenty-three clear days it was found that six occurred between new moon and first quarter, eight before and after full moon, and five from the day of last quarter to the fourth octant. Of the four other clear days, three occurred *in a single year* (1842) on the second day after first quarter, in the months of April, July and August. The amount of cloud in two of the exceptional instances was 0·3.

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In 1844, 1845, and 1846 there were twenty-seven clear days; and of these nearly the same proportion as before occurred at the three periods of greatest depression in the curve, viz. five between new moon and the day before first quarter; ten before and after full moon; and five between the day before last quarter and the last octant. Two of the remaining clear days occurred on the day of first quarter, and four (in 1846) on the day before and day after first quarter*.

In 1847 there were five clear days, of which two occurred after new moon, and one on the second octant. In this last year the observations in January and December were defective.

On an average of the seven years, the proportion of clear, or nearly clear, days (those being considered as nearly clear when the amount of cloud did not exceed 0·5) was 2·2 in each 100 days. Of these, 42 occurred on nineteen days of the lunation at the periods of low mean temperature, and 13 on eleven days at the periods of high mean temperature. Or, if the years 1842 and 1846 be omitted, the average of clear days being reduced to 1·8 in each 100 days, the proportion in which clear days occurred in equal periods of low or high temperature is : : 1·4 : 0·4.

12. Lastly, there are the results of the observations by Schübler at Augsburg, from 1813 to 1828, which were examined by M. Arago and admitted to be in accordance with those made by Flaugergues at Viviers, from 1808 to 1828. From a Table of the relative number of serene and clouded days at Augsburg during the above-mentioned sixteen years, M. Schübler found (1) that clear days were more numerous at last quarter; (2) that the greatest number of clouded days occurred towards (*vers*) the second octant†. Also in twenty-eight years at three different stations, namely at Munich from 1781 to 1788, at Stuttgard from 1809 to 1812, and at Augsburg as above, there were 306 days of rain on the day of the first octant, 325 on the day of the first quarter, 341 (the maximum) on the day of the second octant, 284 (the minimum) on the day of the last quarter, and 290 on the last octant. It will be perceived that these results, though in

* It should be observed that the occurrence of clear days immediately after first quarter, though termed exceptional, is to be considered so relatively only, in the same way that low temperatures are found to occur at the same period without affecting the rise in the curves. Thus out of the twenty-four highest and lowest maximum and minimum mean temperatures for the month at Greenwich, being one highest and one lowest mean temperature for each of the twelve months, during forty-three years, it has elsewhere been shown that *eleven*, viz. seven minima and four maxima, occurred within three days of the first quarter on each side; and of these, five minima occurred before the day of the change, and two after it. Of the maxima, there occurred three after, and one upon the day of first quarter.

† *Annuaire*, 1825, pp. 166 and 169.

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part obtained from observations of an earlier series of years, and at stations far distant from each other, follow the curve of twenty years' mean temperature at Greenwich, and point to a maximum of effects after first quarter, and a minimum at last quarter.

Other evidence is to be found in Howard, who appears to have failed in establishing the fact of lunar influence (in which, from long-continued observations, he had the most perfect belief) mainly from the unfortunate way in which effects, which he nevertheless noticed and commented on, were neutralized by mixing up the means of days, which a very slight glance at the curves will show required to be kept separate.

It should be explained that the vertical lines in the Plate of Curves represent the different days, and the horizontal lines degrees and tenths of degrees of temperature, and also tenths and units of cloud. The dark line intersecting each curve is the mean of the period.

P.S. In the second volume of Admiral Smyth's translation of Arago's 'Popular Astronomy' (p. 313), I find the following passage referring to Sir John Herschel's explanation of the moon's influence on the clouds, which it will be seen he entirely adopts:—

"In a word, provided we do not lose sight of the fact that the rays which dissipate the clouds are quite different from those whose calorific qualities we have been endeavouring to estimate at the instant when they reach the surface of the earth, the fact which I previously called a prejudice will no longer be contrary to physical laws; and we shall obtain an additional illustration of the remark, that popular opinion ought not to be rejected without examination."

I find also in page 318, observations of rain are alluded to, which appear to have been made under M. Arago's personal superintendence:—

"The discussion of the observations made at Paris led to the following conclusions:—

"The maximum number of rainy days is found to lie between the first quarter and the full moon, the minimum between the last quarter and the new moon; and the latter number is to the former as 100 is to 126. The accordance exhibited between the German observations and those made at Paris is, as we have seen, very striking."

M. Arago adds, however, that his assistant, M. Gasparin, found at Orange that the minimum of rainy days occurs between the full moon and the last quarter; but it does not appear whether this was early or late in the quadrant, or what was the duration of the observations. At Montpellier, in 1777, Poitevin also arrived at different results during ten years' observations,

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which, however, may be due to his registering days as rainy on which there was a fall of local mist or mizzle—*légères bruines*. (*Annuaire*, 1825, p. 167.)

I may mention in conclusion, that there appears to be much in the chapters relating to the subject in this second volume to countenance the belief that the moon's surface radiates heat unequally; so that it is perhaps to the different extent of the absorbing surfaces, and the length of time during which, at the several phases, they are exposed to the solar rays, that one may ascribe the difference in the effects which have been noticed. It is evident that the influence must vary with the moon's position; and it may be further subject to other changes, for which the discoveries of M. Nièpce de St. Victor and—I venture to add (from the frequency of storms at certain periods of the moon's age, and the sudden nature of other phænomena)—the experiments of electricians will possibly afford an explanation.

XXIV. *On Regelation, and on the Conservation of Force.* By Professor Faraday.

[The volume of reprinted 'Experimental Researches in Chemistry and Physics,' by Prof. Faraday, which has just been published, contains the following new matter in relation to the above subjects. We think it expedient to transfer it to our pages.]

On Regelation.

THE subject of regelation has of late years acquired very great interest through the experimental investigations of Tyndall, J. Thomson, Forbes and others, and in its present state will perhaps justify a few additional remarks on my part as to the cause. On the first observation of the effect eight years ago, I attributed it to the greater tendency which a particle of fluid water had to assume the solid state, when in contact with ice on two or more sides, above that it had when in contact on one side only. Since then Mr. Thomson has shown that pressure lowers the freezing-point of water*, and has pointed out how such an effect occurring at the places where two masses of ice press against each other, may lead first to fusion and then union of the ice at those places, and so he explains the fact of regelation. Prof. J. D. Forbes† does not think that pressure causes regelation in this manner, though it favours it by moulding the touching surfaces to each other. He admits Person's view

* Belfast Society Proceedings, December 2, 1857.
† Royal Society Edinburgh Proceedings, April 19, 1858.

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of the gradual liquefaction of ice*, and assumes that ice must be essentially colder than ice-cold water, *i. e.* the water in contact with it.

I find no difficulty in thinking it would be easy to arrange a mixture of water and snow in such a manner that it might be kept for hours and days without any transition of heat either to or from it; but I find great difficulty in thinking that the particles of snow, small as they may be made, would remain for the whole of the time at a lower temperature by 0°·3 F. than the particles of water intermingled with them. Still, admitting for the present the possibility that Prof. Forbes's view may be correct, and also the truthfulness of Mr. Thomson's principle, and its possible action in regelation, I wish to say a few words on the other principle already referred to, which was originally assumed by myself, which, in relation with the mechanical theory of heat, has been adopted by Dr. Tyndall, and which, after all, may be the sole cause of the effect.

The principle I have in view being more distinctly expressed is this:—In all uniform bodies possessing cohesion, *i. e.* being in either the solid or the liquid state, particles which are surrounded by other particles having the like state with themselves tend to preserve that state, even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so surrounded, would cause them instantly to change their condition. As water is the substance in which regelation occurs, I will illustrate the principle by the phænomena which it presents. Water may be cooled many degrees below 32° Fahr.† and yet retain its liquid state for, as far as we know, any length of time without solidification; yet, introduce a piece of the same chemical substance, ice, at a higher temperature, and the cold water freezes and becomes warm. It is certainly not the change of temperature which causes the freezing, for the ice introduced is warmer than the water. I assume that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and causes the change. The cold water particles would willingly, as to temperature, have solidified without the ice, but were held fluid by the cohesion with them of other like fluid particles on all sides.

In the other direction, Donny's experiments have taught us that the cohesion amongst the particles of water is so great

* *Comptes Rendus*, 1850, xxx. 526.

† Water may be cooled to 22° F. It is probable that if it were perfectly freed from air it would remain fluid at a much lower temperature; for the air is excluded at the freezing-point, and the occurrence of this exclusion would break cohesion.

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that it will support a column of the fluid four or more feet high when there is no other power to sustain it; or will cause it to resist conversion into the state of vapour at temperatures so much higher than its ordinary boiling- or condensing-point, that explosion will occur when the continuity, and therefore the cohesion, is destroyed. The water may be exalted to the temperature of 270° Fahr. at the ordinary pressure of the atmosphere, and remain as water; but the introduction of the smallest particle of air or steam will cause it at once to burst into vapour, and at the same time its temperature falls.

This ability which water has to retain by cohesion its liquid state, refusing to solidify when below the freezing-point, or to become vapour when above the boiling-point, it has in common with many other substances. Acetic acid, sulphur, phosphorus, many metals, many solutions, may be cooled below the congealing temperature prior to the solidification of the first portions; many other substances, such as alcohol, sulphuric acid, ether, camphine, &c., boil with bumping, or boil with different degrees of facility in vessels of different substances*. The conclusion, that these differences are due to a certain range of cohesion in the case of each body, seems to me both simple and natural; this cohesion enabling the substances to withstand a change of temperature which, without the cohesion, ought to have caused a change of state. The effect of extraneous matters as nuclei also appears to me to be simple; for though when introduced, as into cooled or heated water, their particles may exert a cohesive force (so to say) upon the particles of the fluid, the force so exerted in the first instance is rarely equal to the force exerted between the water particles themselves. Extraneous substances require preparation before their adhesion to fluid is at a maximum; glass will permit water to boil in contact with it at 212°, or by preparation will remain in contact with it at 270° Fahr., as in Donny's experiment. It will also remain in contact with water at 22° Fahr. without causing its solidification, and yet an ordinary piece of glass will set it off at once.

Enough has been said, I think, to show that water particles surrounded by water tend to retain their fluid state in both directions at temperatures which are abundantly sufficient to make it equally retain the solid or the vaporous state when either of them is conferred upon it. There is nothing against the assumption that ice has the like kind of power, *i. e.* the power of retaining its solid state at temperatures higher than the temperature of ice against water. Nevertheless the fact is more difficult to show; still some experiments may be quoted

* Marcet.

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in favour of the view. If hydrated crystals of sulphate of soda, carbonate of soda, phosphate of soda, &c.*, be carefully prepared in clean basins, by spontaneous evaporation of the water, they will retain their form unbroken, and their hydrated state undisturbed, through the high temperatures of a whole summer, though, if broken or scratched even in winter, they will commence to effloresce at the place where the cohesion, and with it the balance of force, was disturbed, and will from thence change progressively throughout the whole mass†. As regelation concerns the condition of water, there is perhaps no occasion to go further. Such facts as the following, however, concern the extension of the principle, and illustrate the power of cohesion, especially in cases where it is coming into activity. Camphor in bottles, or iodide of cyanogen in proper glass vessels, produces crystals sometimes an inch or two in length, which grow by the deposition of solid matter on them from an atmosphere unable to deposit like solid matter upon the surrounding glass, except at a lower temperature. Crystals in solution grow by the deposition of solid matter on them which does not deposit elsewhere in the solution. Many suchlike cases may be produced.

Returning to the particular case of regelation, it is seen that water can remain fluid at temperatures below that at which ice forms, by virtue of the cohesion of its particles; and in so far the change is rendered independent of a given temperature. Next, I rest on the fact that ice has the same property as camphor, sulphur, phosphorus, metals, &c., which cause the deposition of solid particles upon them from the surrounding fluid, that would not have been so deposited without the presence of the previous solid portions,—a fact sufficiently proved by the growth of fine crystals of ice in ice-cold water. This effect was admirably shown in Mr. Harrison's freezing apparatus, where beautiful thin crystals of ice, six, eight, and ten inches long, would form in the surrounding fluid; and these crystals, which could not be colder than the surrounding fluid, exhibited the phænomena of regelation when purposely brought in contact with each other.

The next point may be considered as an assumption: it is that many particles in a given state exert a greater sum of their peculiar cohesive force upon a given particle of the like substance in another state than few can do; and that as a consequence a water particle with ice on one side and water on the other,

* Philosophical Transactions, 1834, p. 74; or Exp. Res. Electricity, vol. i. p. 191, note.

† Such a case shows combined solid water at a temperature ready to separate and change into vapour, yet not changing, because, as far as we can see, the undisturbed cohesion holds all together.

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is not so apt to become solid as with ice on both sides; also that a particle of ice at the surface of a mass in water is not so apt to remain ice as when, being within the mass, there is ice on all sides, temperature remaining the same. If that be admitted, then regelation is sufficiently accounted for. Difference of temperature above or below that of the changing points of water is not alone sufficient to cause change of state, the change being independent of temperature throughout a large range. At such times the particles appear to be governed by cohesion. Cohesion resolves itself into the force exerted on one particle by its neighbours; and this force seems to me to be sufficient, under the circumstances, to account for regelation.

Supposing this to be the true view of the state of things, then a particle of ice within ice can exist at a temperature higher than a like particle of ice on its surface in contact with water; and though it does not appear at present how a higher temperature could be communicated to the interior of a mass of freezing ice than that existing over its surface, still there may be principles of action in radiation, and even in conduction and liquefaction, producing that effect. Assuming, however, that a piece of freezing ice is in such a state, then, if it were to be pulverized, it ought to produce a mixed mass of ice and water colder than the ice was before. Such seems to be the result in one of Prof. Forbes's experiments, in which ice rapidly pounded showed a temperature of 0°·8 Fahr. below the temperature of snow in a thawing state. The experiment, however, would require much consideration in every point of view, and much care before it could be considered as telling anything beyond the temperature of ice-cold water.

On the other hand, if a spherical cup of ice could be prepared containing water within, to which no heat could pass except by conduction through the ice itself, that water ought to be a little colder than the ice cup around it: also if a mixture of snow and water were pressed together, the temperature should rise whenever regelation occurred, being an effect in the contrary direction to that which Prof. J. Thomson contemplates; and such a mixture, as a whole, ought to be warmer than the water in the ice sphere mentioned above. No doubt nice experiment will hereafter enable us to criticise such imaginary results as these, and, separating the true from the untrue, will establish the correct theory of regelation.

On the Conservation of Force.

During the year that has passed since the publication of certain views regarding gravitation, &c., I have come to the know-

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ledge of various observations upon them, some adverse, others favourable: these have given me no reason to change my own mode of viewing the subject; but some of them make me think that I have not stated the matter with sufficient precision. The word "force" is understood by many to mean simply "the tendency of a body to pass from one place to another," which is equivalent, I suppose, to the phrase "mechanical force;" those who so restrain its meaning must have found my argument very obscure. What I mean by the word "force," is the *cause* of a physical action; the source or sources of all possible changes amongst the particles or materials of the universe.

It seems to me that the idea of the conservation of force is absolutely independent of any notion we may form of the nature of force or its varieties, and is as sure and may be as firmly held in the mind, as if we, instead of being very ignorant, understood perfectly every point about the cause of force and the varied effects it can produce. There may be perfectly distinct and separate causes of what are called chemical actions, or electrical actions, or gravitating actions, constituting so many forces; but if the "conservation of force" is a good and true principle, each of these forces must be subject to it: none can vary in its absolute amount; each must be definite at all times, whether for a particle, or for all the particles in the universe; and the sum also of the three forces must be equally unchangeable. Or, there may be but one cause for these three sets of actions, and in place of three forces we may really have but one, convertible in its manifestations; then the proportions between one set of actions and another, as the chemical and the electrical, may become very variable, so as to be utterly inconsistent with the idea of the conservation of two separate forces (the electrical and the chemical), but perfectly consistent with the conservation of a force, being the common cause of the two or more sets of action.

It is perfectly true that we cannot always trace a force by its actions, though we admit its conservation. Oxygen and hydrogen may remain mixed for years without showing any signs of chemical activity; they may be made at any given instant to exhibit active results, and then assume a new state, in which again they appear as passive bodies. Now, though we cannot clearly explain what the chemical force is doing, that is to say, what are its effects during the three periods before, at, and after the active combination, and only by very vague assumption can approach to a feeble conception of its respective states, yet we do not suppose the creation of a new portion of force for the active moment of time, or the less believe that the forces belonging to the oxygen and hydrogen exist unchanged in their

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amount at all these periods, though varying in their results. A part may at the active moment be thrown off as mechanical force, a part as radiant force, a part disposed of we know not how; but believing, by the principle of conservation, that it is not increased or destroyed, our thoughts are directed to search out what at all and every period it is doing, and how it is to be recognized and measured. A problem, founded on the physical truth of nature, *is stated*, and, being stated, is *on the way* to its solution.

Those who admit the possibility of the common origin of all physical force, and also acknowledge the principle of conservation, apply that principle to the sum total of the force. Though the amount of mechanical force (using habitual language for convenience sake) may remain unchanged and definite in its character for a long time, yet when, as in the collision of two equal inelastic bodies, it appears to be lost, they find it in the form of heat; and whether they admit that heat to be a continued mechanical action (as is most probable), or assume some other idea, as that of electricity, or action of a heat-fluid, still they hold to the principle of conservation by admitting that the sum of force, *i. e.* of the "cause of action," is the same, whatever character the effects assume. With them the convertibility of heat, electricity, magnetism, chemical action and motion is a familiar thought; neither can I perceive any reason why they should be led to exclude, *a priori*, the cause of gravitation from association with the cause of these other phænomena respectively. All that they are limited by in their various investigations, whatever directions they may take, is the necessity of making no assumption directly contradictory of the conservation of force applied to the sum of all the forces concerned, and to endeavour to discover the different directions in which the various parts of the total force have been exerted.

Those who admit separate forces inter-unchangeable, have to show that each of these forces is separately subject to the principle of conservation. If gravitation be such a separate force, and yet its power in the action of two particles be supposed to be diminished fourfold by doubling the distance, surely some new action, having true gravitation character, and that alone, ought to appear, for how else can the totality of the force remain unchanged? To define the force as "a simple attractive force exerted between any two or all the particles of matter, with a strength varying inversely as the square of the distance," is not to answer the question; nor does it indicate or even assume what are the other complementary results which occur; or allow the supposition that such are necessary: it is simply, as it appears to me, to *deny* the conservation of force.

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As to the gravitating force, I do not presume to say that I have the least idea of what occurs in two particles when their power of mutually approaching each other is changed by their being placed at different distances; but I have a strong conviction, through the influence on my mind of the doctrine of conservation, that there is a change; and that the phænomena resulting from the change will probably appear some day as the result of careful research. If it be said that "'t were to consider too curiously to consider so," then I must dissent: to refrain to consider would be to ignore the principle of the conservation of force, and to stop the inquiry which it suggests,—whereas to admit the proper logical force of the principle in our hypotheses and considerations, and to permit its guidance in a cautious yet courageous course of investigation, may give us power to enlarge the generalities we already possess in respect of heat, motion, electricity, magnetism, &c., to associate gravity with them, and perhaps enable us to know whether the essential force of gravitation (and other attractions) is internal or external as respects the attracted bodies.

Returning once more to the definition of the gravitating power as "*a simple attractive force exerted between any two or all the particles or masses of matter at every sensible distance, but with a* STRENGTH VARYING *inversely as the square of the distance,*" I ought perhaps to suppose there are many who accept this as a true and sufficient description of the force, and who therefore, in relation to it, deny the principle of conservation. If both are accepted and are thought to be consistent with each other, it cannot be difficult to add words which shall make "varying strength" and "conservation" agree together. It cannot be said that the definition merely applies to the *effects* of gravitation as far as we know them. So understood, it would form no barrier to progress; for, that particles at different distances are urged towards each other with a power varying inversely as the square of the distance, is a truth: but the definition has not that meaning; and what I object to is the pretence of knowledge which the definition sets up when it assumes to describe, not the partial effects of the force, but the nature of the force as a whole.

XXV. *On a Method of Observation applied to the study of some Metamorphic Rocks; and on some Molecular Changes exhibited by the action of Acids upon them.* By ALPHONSE GAGES, *M.R.I.A.**

CHEMICAL analysis makes us acquainted with the constituents of rocks, and with the relative proportions in which they are combined; but, generally speaking, it can tell us no-

* Communicated by the Author, having been read at the Meeting of the British Association at Leeds.

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thing of their origin, mode of formation, or intimate structure. The various reactions to which we must have recourse give us the elements of which rocks are composed, but usually in a state of combination wholly different from that in which they previously existed. Chemical analysis may enable us to form perfectly clear conceptions about the nature of definite mineral compounds, but by its aid alone we could not hope to arrive at any very certain results as to the character of the modifications which such compounds might undergo in time. If this be true of simple minerals, how much more so must it be of such heterogeneous substances as the majority of rocks, especially those of metamorphic origin.

The mechanical processes employed in the preparation of fragments of rocks for analysis, as well as some of the chemical operations to which the mechanically prepared substance must be submitted, destroy the peculiar structural arrangement of the rock, and intimately mingle different constituent minerals, or the altered and unaltered part of the same mineral. It is only by a series of comparative experiments, varied in every possible way, that we could hope to solve the problem of the genesis of many minerals and rocks, but particularly of the class known as metamorphic.

The simple action of acids and other dissolvents on many rocks, removing from them certain parts and leaving others exposed to view, affords us the opportunity of making such a series of comparative experiments as may often enable us to discover their mode of formation, and the character and extent of the alteration they may have suffered. It is important to remark that the mechanical state of the substances to be acted on is not an indifferent element in experiments of this kind; the chemical result will of course be the same, whether the substances to be acted upon be in the form of powders, of laminæ more or less fine, of rock fragments, or of crystals cut in the direction of some of their cleavage planes; but the true interpretation of the several phænomena observed will be essentially different according to the geological origin of the substances under investigation. In support of this proposition I may allude to some examples lately supplied by experiments which I have made, and of which the following notice contains an outline. In carrying out these experiments, I have paid special attention to the skeleton which results from the action of acids upon thin laminæ of rocks or individual crystals.

One of the best examples of the value of this mode of examination by acids was afforded by a fibrous dolomite, found near Miask in the Ural Mountains. The analysis of the mineral made in the ordinary way gave a quantity of lime, magnesia, and silica,

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represented by the following numbers:—

Carbonate of lime	57·483
Carbonate of magnesia	40·974
Sesquioxide of iron and alumina . .	0·411
Water and organic matter	0·239
Silica	1·095
	100·202

From this analysis it would appear that the mineral to which I have referred is a dolomitic rock; but it affords us no information whatever relative to its real nature or origin. If, however, instead of operating on the mineral in the form of powder, or fragments coarsely broken up, we proceed by means of diluted hydrochloric acid acting on a single fragment of moderate dimensions cut in the direction of the fibres, we shall observe, after continuing the process for some days, that there will be left an asbestiform skeleton having the following composition:—

Silica	68
Magnesia*. . . .	29

—numbers representing a magnesian tremolite. It is from this simple difference in the manner of conducting the experiment that a result so different from the former has been arrived at, and one that enables us to trace, so to speak, the real origin of the rock in question.

Some varieties of magnesite (siliceous carbonate of magnesia) similarly treated with dilute hydrochloric acid, leave silico-gelatinous residues, which afford indications, as in the former case, of the origin of the rock from which they are derived. Another case in point is the possibility of following the transition from meerschaum, which is a definite silicate of magnesia, to a replacing pseudomorphite of ordinary carbonate of magnesia containing mere traces of silicates.

Another example is afforded by the manner in which concentrated hydrochloric acid acts upon crystals of certain varieties of zeolites with alkaline bases. This action shows, from the partial decomposition effected, the stages of alteration through which these minerals pass. A crystal of Thomsonite boiled with hydrochloric acid, deposits, after the saturation of the alkali, a gelatinous transparent precipitate of silica; and an opaline skeleton remains, which, to a certain extent, presents the outlines of the primitive crystal. A mass of these crystals treated in the same manner, and dried after separation of the gelatinous silica, resembles in appearance some of the siliceous aggregates which are often found in solfataras and in other volcanic localities as well

* *Vide* Phil. Mag. for March 1858.

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as in some thermal springs. No doubt many of those volcanic products have been formed by natural processes similar to that which I have described.

The opals found in the basaltic district of the North of Ireland, and occurring in the cavities of amygdaloidal greenstone at the Giant's Causeway, have most probably been derived from the gelatinous silica of decomposed zeolites, while some of the specimens of the same locality described as hydrophane, resemble in a remarkable manner certain of the skeletons resulting from the action of acids upon some altered trap-rocks*.

I may, as an illustration, refer to a variety of magnesite derived from the decomposition of basalt, and described by General Portlock in his 'Geological Report on the Counties of Londonderry, Tyrone, and Fermanagh,' pp. 114 and 115. This mineral substance presents one of the best illustrations of the peculiar metamorphic changes which occur during the decomposition of some basalts. Dr. Apjohn has given an analysis of the mineral at page 114 of the 'Report' above named, from which it would appear to be a hydrous silicate of alumina and magnesia. This mineral substance is of a greyish-white colour, and consists of a series of parallel laminæ.

A lamina of this mineral, of about 2 centimetres square by 2 millimetres thick, when boiled for some time with hydrochloric acid and then with sulphuric acid, leaves, after an action of several days, a skeleton of amorphous silica, blackened by the sulphuric acid acting upon organic matter derived, doubtless, from the water of infiltration. When the acid and organic matter are removed by washing and ignition, there remains a skeleton of pure amorphous silica lighter than water, and presenting the perfect form of the primitive substance, visibly constituted of thin laminæ superimposed like the leaves of a book. After immersion in water for a sufficient length of time, it becomes translucent, and acquires all the characters of certain varieties of hydrophane. The quan-

* Common opal occurs, filling the cavities of amygdaloidal greenstone at the Causeway; Rathlin Island; Crossreagh, parish of Ballywilling; and in several places along the basaltic range,—principally white, varying from translucent on the edges to opake; also striped, and sometimes yellowish.

At Sandy Braes (Connor parish, County of Antrim), it is met in great abundance, forming strings or irregular veins in the pitchstone porphyry of that district, generally opake and white; also yellow, or reddish yellow, and highly translucent, having much the aspect of the coarser varieties of amber. Occasionally it presents considerable play of colours: associated with it, but sparingly, jaspar-opal is found.

Hydrophane is found at the Causeway in small roundish masses in amygdaloid, of a brownish-white colour, much like mountain-cork. Also at Crossreagh, parish of Ballywilling. (Report on the Geology of the County of Londonderry, Tyrone, and Fermanagh, by General Portlock, F.R.S.)

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tity of water which it absorbs is more than 11·5 per cent. If left exposed to the air for some time, the siliceous skeleton loses the greater part of the water, but retains a mean quantity of about 6·40 per cent., which corresponds very nearly with the formula $3Si, O^3, HO$ given by Beudant for an opake white opal from Castellamonte.

If it be immersed in a solution of ammoniacal sulphate of copper, and afterwards exposed to the open air, it retains a portion of the copper salt, even though subjected to repeated washings, and in the moist state it presents the appearance of certain varieties of silicate of copper (copper hydrophane); in sulphuric acid the substance becomes hyaline, and retains a portion of the acid with great tenacity even after repeated washings. A solution of caustic potash dissolves the skeleton with great facility even after ignition.

Although it seems difficult to ascribe to this siliceous matter a capability of forming definite compounds, yet the facts just mentioned are not less remarkable. The molecular condition in which the silica exists in such alumino-magnesian compounds as I have described, and the action which it exerts on a great number of substances, would appear to indicate a point of connexion between chemical phænomena, strictly so called, and cohesive forces*.

As an illustration of the decomposition and subsequent reconstruction of rocks, I may refer to a pseudomorphite of quartz-rock, in all probability derived from magnesite, and obtained from nearly the same locality as the former mineral, which,

* Sir J. Herschel, in his Introductory Address as President of the Chemical Section of the British Association at the Leeds Meeting, 1858, made some observations on the relation between capillarity and chemical affinity, which appear to me to have a striking bearing upon the above experiments. I cannot avoid quoting the following passage from that discourse:—

"There is another class of phænomena which, though usually considered as belonging peculiarly to the domain of general physics, and so out of our department, seems to me to want some attention in a chemical point of view. It is that of capillary attraction. The coefficient of capillarity differs very remarkably in different liquids, and no doubt also in their contact with different solids,—a fact which can hardly be separated from the idea of some community of nature between the capillary force and those of elective attraction. I hardly dare to hint at the existence of some slight misgiving I have always felt as to the validity of the received statical theory of capillary action, which carries with it the authority of such names as those of Laplace and Poisson. Any discussion of this point would be matter for another section of this Association; and if I here touch upon it, it is only to observe that my impression of the requisiteness of a force *so far allied to chemical affinity as to be capable of saturation*, rests on other grounds besides that of the mere diversity of action above alluded to."

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like it, was composed of a series of parallel laminæ: the interior layers possessed a certain amount of permeability, which, upon examination with a lens, showed that silica had passed into the crystalline state; nevertheless some traces of amorphous silica could still be detected by the test of caustic potash. The density of this pseudomorph is nearly the same as that of ordinary quartz-rock. It would appear, therefore, that the alumino-magnesian base of the original rock having disappeared, a permeable siliceous skeleton remained, which was subsequently infiltrated by the silica of the alkaline silicates derived from the decomposition of the surrounding trap-rocks.

To the same class of phænomena we may probably also refer the petrifaction of the fossil wood occurring in the vicinity of Lough Neagh, and which, according to Bischof, contains 71 per cent. of silica; and the slight alkaline reaction which the same observer has attributed to the waters of that lake, may in such case be attributed to the decomposition of the alkaline silicates.

The mineral substances called mountain leather and mountain cork, which are chiefly derived from the decomposition of hornblendic and augitic rocks, as in the county of Londonderry, exhibiting as it were in themselves a kind of natural process similar to that here described, leave, when treated with acid, a white spongy skeleton of excessive lightness, which swims in water and bears the greatest analogy to some varieties of *nectique* quartz. This residue of silica absorbs about four times its weight of water, and rapidly dissolves in a weak solution of caustic potash, even after the skeleton has been heated to redness. The specimens of mountain leather, cork, &c., from the district above named, as well as many minerals of a like character, are evidently the result of a more or less advanced state of alteration of hornblendic and augitic rocks. We may recognize two stages of this decomposition: in the first, we have sometimes almost a mere spongy aluminous silicate, of variable composition, often more or less impregnated with carbonates of lime and magnesia, or with carbonate of lime alone; in the second stage the whole of the siliceous compounds forming the sponge disappear, and are replaced by carbonates of lime and magnesia, or by both—the character of the metamorphosis being often only recognizable by thin coatings, often mere films, of mountain-leather substances, covering one or both sides of the replacing carbonates: these films are always recognizable by a practised eye.

I may here remark that the silica retained in the preceding substances is always in the soluble amorphous condition, which appears to be conclusive as to their origin.

Serpentine, cut into thin pieces of various shapes and treated by acids and other solvents, exhibits in a great number of in-

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stances the original mineral substances from which it has been derived.

By this treatment, a siliceous skeleton is always left, in which the mineral substances alluded to are nearly always enclosed; on immersion in water, this skeleton becomes opalescent, and exhibits many of the phænomena of the skeletons already described.

This mode of treatment by various solvents enables us at once to account for the variations which occur in many of the published analyses of serpentine, as it must be evident that the nature, quantity, and stage of decomposition of the enclosed minerals must greatly vary. My experiments on this subject are not as yet sufficiently advanced to offer more than a few concluding remarks respecting some properties which I have lately observed in connexion with the skeletons of several specimens of serpentine. In two I have succeeded in detecting the presence of organic matter under the action of concentrated sulphuric acid. One of these is from the neighbourhood of Holyhead, and the other from Snarum in Norway. The specimen from Norway is of a yellowish-green colour with undulating lines of light green; and its skeleton, which is almost entirely soluble when submitted to the action of caustic potash, shows small micaceous spangles of talc, and also black spots derived from decomposed garnets. It absorbs about 40 per cent. of water, becoming translucent, and is of a rather compact structure. The serpentine from the neighbourhood of Holyhead, treated with sulphuric acid, presents marks of carbonization; but it is only at certain isolated points, and the skeleton appears to possess the property of cleavage in certain definite directions.

The serpentine of Galway, though of a variable character, is more or less readily acted upon by acids, according to the state of alteration which it has undergone, and the quantity of carbonate of lime which it contains, the latter being often uniformly disseminated through the mass. The skeleton which it leaves is in general very friable, and falls to powder on drying; the part not affected by the action of caustic potash is formed by an agglomeration of micaceous spangles of talc, and also insoluble silica. The green colour is produced chiefly by protoxide of iron, which forms the irregular veins observable.

In the serpentine of Penzance, coloured chiefly by peroxide of iron, the siliceous skeleton envelopes a nucleus almost unaffected by the action of acids, and which consists of an aggregation of more or less altered diallage and hornblende.

By the simple process which I have endeavoured to describe, namely the submission of thin laminæ to the influence of acids and other solvents, true serpentines, which are hydrated mine-

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rals leaving siliceous skeletons of amorphous silica, may be distinguished at once from many other rocks, frequently considered as serpentine. The latter are often mere altered clay-slate, so nearly resembling true serpentine lithologically, as to have been frequently confounded with them, though quite distinct in chemical composition.

The only object I have had in view in the preceding notice, has been to direct the attention of geologists to the method; as I am still occupied with the subject, I have thought it better to reserve fuller details for another occasion.

XXVI. *The* Astronomer Royal's *Remarks on* Mr. Cayley's *Trigonometrical Theorem, and on* Professor Challis's *Proof that Equations have as many Roots, &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE following are the partly geometrical proof and the geometrical interpretation of Mr. Cayley's equation of spherical trigonometry,

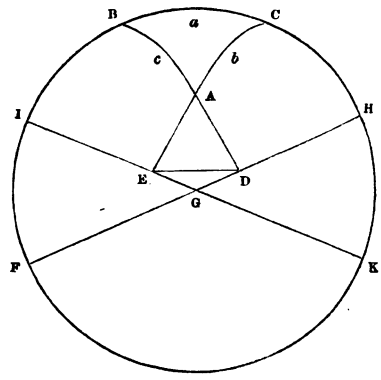
$$\sin b \,.\, \sin c + \cos b \,.\, \cos c \,.\, \cos A = \sin B \,.\, \sin C - \cos B \,.\, \cos C \,.\, \cos a,$$

given in your last Number.

In the diagram, let the hemisphere be so projected that its boundary is the great circle formed by the side a produced. Produce b and c in arc of great circle, so that

$$BD = CE = 90°,$$

and join DE by an arc of great circle. Then each side of the equation above will be equal to cos DE.



First. $\cos DE = \cos AD \,.\, \cos AE + \sin AD \,.\, \sin AE \,.\, \cos A = \cos (90° - c) \,.\, \cos (90° - b) + \sin (90° - c) \,.\, \sin (90° - b) \,.\, \cos A = \sin c \,.\, \sin b + \cos c \,.\, \cos b \,.\, \cos A.$

Second. With pole B describe the great circle F G D H, and with pole C describe the great circle I E G K; G being the intersection of the two great circles. Since $GB = GC = 90°$, G is the pole of a, or the centre of the projection. Therefore

$$GE = GI - IE = 90° - C; \text{ and } GD = 90° - B.$$

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Also $BF = 90° = CI$; therefore $IF = BC = a$; and

$$EGD = 180° - IGF = 180° - a.$$

Hence

$$\begin{aligned}\cos DE &= \cos DG \cdot \cos EG + \sin DG \cdot \sin EG \cdot \cos EGD \\ &= \cos (90° - B) \cdot \cos (90° - C) \\ &\quad + \sin (90° - B) \cdot \sin (90° - C) \cdot \cos (180° - a) \\ &= \sin B \cdot \sin C - \cos B \cdot \cos C \cdot \cos a.\end{aligned}$$

Consequently

$$\sin b \cdot \sin c + \cos b \cdot \cos c \cdot \cos A = \sin B \cdot \sin C - \cos B \cdot \cos C \cdot \cos a,$$

which is Mr. Cayley's theorem.

I take this opportunity of offering a remark on Professor Challis's "Proof that every Equation has as many Roots," &c. in your last Number.

The sentence which commences on page 113 and terminates on page 114, stands thus:—"Hence after eliminating z from the above two equations, we may obtain a possible value of y^2 from the resulting equation by methods of approximation." I submit that at present we are not entitled to assume that we can obtain a possible value of y^2 from the resulting equation. If it can be shown that the resulting equation is of an odd order; or if it can be shown that, if of an even order, its last term is negative; then it will be certain that a value of y^2 can be found. But till one of these cases is proved, I submit that the possibility of finding a value for y^2 is not proved.

I am, Gentlemen,

Your obedient Servant,

G. B. Airy.

Royal Observatory, Greenwich,
February 4, 1859.

XXVII. *On the Reflexion and Inflexion of Light by Incandescent Surfaces.* By W. R. Grove, *Esq.*, *F.R.S.* &c.

To W. Francis, Esq.

My dear Sir,

ON putting in order some old papers, I found a manuscript in my own handwriting, and the subject of which I had entirely forgotten; and it was not until some time had elapsed that I could recollect anything about the experiments contained in it. I now remember that they were made at the London Institution; and it must have been from ten to fifteen years ago. I have no recollection of the reason why I did not

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publish them, and can only guess that it was in accordance with my general habit of not publishing negative results. The results here, however, though negative, seem to me interesting, as positive results would *à priori* be expected; and if you think them worth publishing in the Philosophical Magazine they are at your service.

Yours faithfully,
W. R. Grove.

The difference in appearance to an observer of a polished surface when at ordinary temperatures and when ignited, is sufficiently marked. The self-luminous character of the ignited body apparently removes the impressions of surrounding objects, and would lead to the belief that reflexion, at least that of the character yielded by polished surfaces, was destroyed. Such has been the *à priori* impression of those whose opinions I have asked on the subject. My own belief was, that if polished surfaces when ignited reflected light, they at all events broke up or scattered the reflected rays, and would cease to have the character of a polished surface; and that if they reflected light at all or notably, they would reflect it as paper or snow does, dispersing the rays so as to produce a general impression of luminosity, instead of throwing them back in a parallel beam, or one in which they preserved their original relative inclination.

The subject appeared worth investigation; and as I could not find that it had been attempted, I determined to make a few experiments upon it. The difficulty which immediately presented itself was, that the surfaces which are mainly employed for polished reflexion being oxidable metals, their physical structure would be changed by the oxidation consequent on incandescence. Gold or platinum, therefore, were the only substances which promised any success; and the latter, from its reflecting white light and more ready capability of retaining a high temperature, was selected. A strip of platinum-foil, 2 inches long by 0·2 broad, was firmly stretched on a piece of plate glass, polished with putty powder and tripoli until it had reached as high a lustre as it could be made to attain. One extremity was then fixed in a clamp attached to a wooden frame; and to the other extremity was attached, by a similar clamp, a metal weight, from which weight a wire extended and dipped into a vessel of mercury: the whole was arranged with care, so as not to bend or disturb the plane surface of the platinum. The foil thus suspended was brought opposite a vertical cleft in a window-shutter facing the meridian, which cleft could be made of any convenient size by horizontally moveable boards. The platinum-foil was placed opposite the cleft, so as to receive a sunbeam; a sheet of

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white paper was arranged directly in the path of the reflected beam; and the distance of the paper from the platinum was indefinitely varied during the experiment. Having accurately marked the boundaries of the reflected beam and its intensity, as far as the eye could judge, the platinum was made part of the circuit of a voltaic battery, the intensity of which was varied so as to produce effects on the foil varying from a heat scarcely visible in the dark, to incandescence up to the point of fusion, or rather to the point at which the foil broke, from its diminished cohesion; for with a weight suspended, although not more than barely sufficient to keep the foil stretched, it always broke off at a temperature short of its point of fusion. In none of these variations, however, was there the slightest apparent difference in the reflected light on the paper. Or if, as occasionally happened, the shape a little changed during the progress of the experiment, it was fully explained by the elongation dependent upon the heat, or by the consequent removal of slight curvatures.

A similar experiment was made with diffused daylight, and with similar effects; also with the light from an Argand lamp. In the latter case, when the reflected beam was so dim as to be interfered with by the light afforded by the incandescent platinum, the image was proportionately affected. Still it preserved its character, and, as far as could be judged, its intensity; and it was only by a very high degree of incandescence and a very feeble incident light that the reflected image seemed to merge in the direct light from the incandescent body.

I now brought my eye into the position where the paper had been placed so as to catch the reflected beam, while my assistant alternately made and broke contact with the battery. When the incident sunlight was sufficiently intense to mask the emitted light of incandescence, I could not in the slightest degree distinguish whether the platinum was ignited or cold. When I first tried it, I two or three times complained wrongly to my assistant that he had not made contact when I told him to do so; when the incident light was very dim, the emitted light was of course also distinguishable. I now caused the spectrum from a flint-glass prism to fall on the platinum, and with similar effect; *i. e.* when the reflected spectrum was very intense, no difference could be detected between the light from the platinum, whether cold or ignited, or whether received upon paper or upon the eye; when less intense, the red portion of the spectrum was elongated by the light of incandescence, and the other portions partook of the character of the spectrum superposed upon, or blended with, the light of incandescence.

The prism was also arranged so as to intercept the reflected instead of the incident beam; the effects were similar.

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A beam of light polarized by reflexion at the proper angle from a plate of glass, was made to fall on the platinum surface, and then analysed by a tourmaline; no difference was perceptible in the plane of polarization, whether the platinum was ignited or not.

The light reflected from the platinum was similarly polarized and analysed; but no difference dependent upon incandescence was detected.

A wire of platinum, 6 inches long and $\frac{1}{80}$th of an inch in diameter, was vertically suspended in the narrow fissure of the shutter; the bands of interference were received on paper placed at different distances from the wire, and examined both by the eye and by a lens; no difference could be detected in these bands when the wire was ignited by a voltaic battrey.

In all the above experiments the foil or wire was ignited by the battery previously to the commencement of each class of experiment, so as to avoid any effect arising from the alterations caused by the platinum having been subjected to heat,—such, for instance, as the effect of annealing might produce, or the burning off from it of films of moisture, or of oxidable substances.

The general result of these experiments is, that no difference is perceptible by the eye in light reflected by a polished surface, whether that surface is ignited or not; that the superficial molecular uniformity which causes a bundle of parallel rays to preserve their parallelism of direction when reflected, is, if the ignited substance be inoxidable, not broken up by ignition.

I know of no photometer which would be suitable for indicating their effects with greater accuracy than the eye; but, although these results lead to a conclusion different, I believe, from that which would have been arrived at *à priori*, they by no means exclude the possibility or even probability of some difference being produced in the direction or character of light reflected from ignited surfaces as compared with that reflected from unignited surfaces.

The fixed lines in the spectrum, for instance, differ materially according to the source of light; and even supposing the ignited surface to make no difference in the character or position of the fixed lines of reflected solar light, a point which I have no apparatus sufficiently delicate to detect, yet there is every probability of novel and valuable results being attained by the interference of this light of incandescence with that of solar or other light reflected from the incandescent body, the same body being then in some sense the source of two different descriptions of light, which differences are capable of detection by the different position and character of the fixed lines in their respective spectra.

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Such experiments, and many others which they obviously suggest, appear to me to offer an interesting field of experiments in physical optics; and to those who are more practically acquainted with this science than I can pretend to be, and who may possess more delicate means of detecting minute effects, I therefore leave them.

XXVIII. *Terrestrial Climate as influenced by the Distribution of Land and Water at different Geological Epochs. By* Henry Hennessy, *F.R.S., M.R.I.A., Professor of Natural Philosophy in the Catholic University of Ireland**.

EVERY point on the earth's surface is continually gaining and losing heat; and its actual temperature at any given moment depends on the difference between its gains and its losses. If the outer coating of the earth were exclusively composed of solid materials, terrestrial climate would depend principally on the heat gained from sunshine and the heat radiated into space. But as the earth is completely enveloped by an atmosphere, and partly surrounded by a liquid, its thermal conditions must be greatly influenced by the physical properties of these fluid coverings. While the heating or cooling of a solid follows the clearly defined, and comparatively well understood, laws of conduction and radiation, the heating or cooling of gases and liquids is further greatly modified by the mobility of their particles. The changes of state which frequently take place in fluids, whether by evaporation or condensation, freezing or liquefaction, introduce agencies which still further complicate the study of their thermal relations.

When we study the thermal conditions of a liquid distributed over the terrestrial spheroid, it becomes manifest that these conditions are influenced by the area, configuration, and physical structure of such portions of the solid earth as rise above the ocean and come in contact with the atmosphere, so as to constitute the surface of the dry land. Upon this matter I propose to develope certain views which are closely connected with those I have already published relative to the distribution of heat over such solid surfaces†.

2. When a surface, covered with ordinary soil, receives the rays of the sun, the heat thus acquired passes downwards, but on arriving at a very small depth its intensity rapidly diminishes.

* From the *Atlantis* for January 1859; communicated by the Author.

† On the Distribution of Heat over Islands, &c., Phil. Mag. for October 1858, p. 241. See also the Note on the Laws that Regulate the Distribution of Isothermal Lines, *Atlantis*, No. 3. p. 201.

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The solar heat which is thus received by the ground may therefore be considered as confined almost entirely to a thin superficial stratum. The air in contact with the soil becomes heated, expands, and tends to ascend: a circulation thus follows between the upper and lower strata of the atmosphere situated above the heated ground. During the night a different process takes place; for then the radiation of the soil causes its temperature to fall below that of the superincumbent air; the coldest stratum of the lower portions of the atmosphere being in contact with the ground, the equilibrium of those above is not so much disturbed. Yet, even in this case, causes exist which tend to produce a series of actions and reactions between the upper and lower strata of air, by which a process of convection will be ultimately developed. These actions will be rendered especially remarkable if the soil is not bare, but covered with vegetation in the manner of the greater part of the dry land. This question has been fully treated by Melloni*, in his memoir on the nocturnal cooling of bodies. His general proposition, that "a body exposed during the night to the influence of a sky of equal clearness and calmness, is always cooled to the same extent, whatever may be the temperature of the air," is fruitful in important results. Thus is explained the great differences between the temperature of the day and night on land in the torrid zone. The intense cold observed during the night by Denham in traversing the great Desert of Sahara, the process of artificial freezing at Bengal, and the rain-like dews observed by Humboldt in the forests of South America, are all necessary consequences of the energy of the actions and reactions by which the outer coating of the earth loses the warmth it has acquired from sunshine during the day. Conversely, the almost constant temperature of the sea in tropical regions, by day and night, and the nearly total absence of dew on the rigging of vessels far removed from the land, clearly show the peculiar retentiveness of heat possessed by the water, and that, unlike the land, it does not readily part with whatever warmth it may have acquired from sunshine during the day. The cold southerly breezes sometimes observed in Egypt† during the winter months, when the air has passed over immense surfaces of sandy desert, present a striking contrast to the south-westerly winds which at the same season traverse the ocean and visit our shores. It appears, from a communication in the *Times* newspaper, dated Melbourne, November 15, 1858, that in South Australia, the coldest winds during the winter months are those blowing from the northerly and tro-

* Taylor's Scientific Memoirs, vol. v. pp. 453 and 530; and *Annales de Chimie et de Physique* for February and April 1848.

† Kaemtz, *Météorologie*, French edit. p. 45.

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pical regions, while the warmest are those blowing from the pole. The former pass over extensive surfaces of heat-radiating, and therefore heat-losing land, while the latter traverse the heat-retaining ocean. In the summer (at least by day) the opposite phænomena are observed, of warm winds from the north and cold from the south. Combined observations on the wind and on temperature, by day and night, would further elucidate a problem which, in the words of the writer, "cannot be solved without greatly adding to the stock of our knowledge." While the feeble conducting power of the solid portions of the earth's coating allows but a small portion of the sun's heat to pass beneath the surface, so that whatever warmth is thus received on that surface during the day is readily radiated into space during the night, a liquid mass, similarly exposed to sunshine and subsequent nocturnal radiation, possesses peculiar properties which greatly influence the differences between its thermal losses and gains. The most important of these properties are—(1) the great capacity of water for heat, by which it gradually accumulates and slowly parts with whatever warmth it has received; and (2) the intermobility of its particles, by which exchanges of temperature in different parts of the liquid mass are essentially promoted.

Let us consider the effect of the sun's rays on a globe covered with water, and we shall soon perceive that a more energetic process than that of conduction accompanies the exchange of temperature between the different portions of the fluid. The water which receives the vertical rays of the sun will be more heated than the waters which receive its rays at more oblique inclinations. Not only the amount of warmth received over a given area, but also the depth to which the rays of heat penetrate below the surface, depends upon the angles made by these rays with the vertical. Inequalities of surface temperature, depending on the latitude, the hour-angle, and the sun's longitude, should thus result. The more heated waters would expand, and tend to spread over the cooler waters in other regions. Currents should arise from the mutual actions and reactions of the unequally heated portions of the finid. The colder currents would usually tend to flow beneath the warmer, unless at temperatures approaching that of the maximum density of water, and thus a process of circulation would be established by which the temperature acquired by the superficial strata of the water should be ultimately propagated to a certain depth below the surface. Evaporation would also take place, and by the condensation of vapour a certain portion of the heat received by the water would be imparted, in the formation of clouds, to the superincumbent atmosphere.

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If, as in the existing oceans, this water be salt, the inequalities of temperature, producing inequalities of evaporation, will also produce diversities in the density of the water in different regions, and thus additional energy will be imparted to the process of circulation. The salter and heavier surface water will tend to sink into the colder liquid which lies beneath, and which will naturally tend to take its place, by ascending upwards*. The process of evaporation would cool the surface of the water; but, unlike that of radiation, it is not altogether a losing process so far as the entire surface of the earth is considered; for it is sooner or later followed by condensation, whereby the greater part of the absorbed heat is again returned. When a piece of land or water parts with its heat by radiation into space, that warmth can never be restored to any part of the earth's surface; but whatever heat the water loses by evaporation, becomes latent in the vapour so produced, and is ultimately transferred by condensation to some other part of the globe; and hence evaporation does not constitute an agent in causing a diminution of general terrestrial temperature. Let us now suppose a sheet of water at the equator nearly surrounded by fixed boundaries, so as to form a species of immense lagoon. Its temperature, from the causes here referred to, will rapidly augment. The heat which it has acquired during the day will have penetrated so deeply as to be incapable of being radiated backwards into space, during the night, with the same facility as on the surface of a sandy plain or from the summits of a mass of vegetation. Its temperature should thus continue to accumulate up to a certain limit imposed by the conditions of evaporation; and it might ultimately attain a mean temperature superior to any which is now met at the surface of intertropical seas.

3. These views are strikingly illustrated by the phænomena accompanying the origin of the Gulf-stream. The mass of water which rushes into the Gulf of Mexico, along the southern shores of the Caribbean Sea, has already acquired a certain elevated temperature from the action of sunshine in the southern torrid zone in its passage from Cape St. Roque. In moving around the Caribbean Sea and the Mexican Gulf, these waters still continue under the influence of a tropical sun, and are constantly increasing in temperature. The islands and coasts which they happen to bathe, have no part in directly promoting this augmentation. On looking over the isothermal chart of the Caribbean Sea and Gulf of Mexico, prepared by M. Charles Deville†, it becomes manifest that in general the temperature decreases in

* See Maury, 'Physical Geography of the Sea,' p. 160.

† *Annuaire de la Société Météorologique de la France*, tom i. p. 160.

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going towards the land. In some places the mean annual temperature of the water close to the land is 24°·5 Centigrade; further out at sea it is 25°, and still further from the land it is 25°·5. In other places it gradually augments from 26°, in going from the land, up to 27°·4*. These results are unconnected with the influence of latitude; and they are still less explicable by the influence of centrifugal force, in driving the cooler and heavier waters towards the edges of the great current, in its semi-rotatory movement around the gulf; for in this case the law of decrease of temperature in going from the land, should not hold on approaching the coasts of large islands situated towards the centre of the moving mass of waters. But in such instances it is also manifested; for on the north and south coasts of the Island of Cuba we find the isothermal lines of 26°·2 and 26°·5, while the isothermals of 26°·7 and 26°·8 are situated outside them respectively†. In M. Deville's chart these are closed isothermals, similar to those which I have indicated on the surface of the British Islands; but as the lowest isothermals in my map are the most remote from the sea, so those in his chart which exhibit the highest temperature are furthest from the land. It is thus apparent that the intertropical sea may become a storehouse of heat, by retaining much of what it receives from the sun, which, but for the physical properties of water, it would, like the intertropical land, lose by radiation into space. It is important to bear this conclusion in mind in any inquiries respecting the influence of the distribution of land and water on general climate, especially as the influence of the land seems to have been hitherto principally considered as a calorific agent.

The heating action of intertropical land has been so often discussed by writers on climate, that it is unnecessary to do more than to point out its principal agency in the production of aërial currents, by which exchanges of temperature may be promoted between different parts of the earth's surface.

In contrasting the mean temperature of the sea with that of the land in tropical climates, the want of nocturnal observations, as referred to by M. Melloni, is peculiarly felt. While the temperature of the one is nearly constant, that of the other is liable to considerable fluctuations; and as our records are principally derived from diurnal observations, the results are probably too favourable to an excess of land temperature. This conclusion is confirmed by the results exhibited in M. Deville's map, and in some measure by the fact of the higher mean temperature of

* Reduced to degrees of Fahrenheit's scale, these numbers, arranged in the same order as in the text, are 76°·1, 77°·0, 77°·9, 78°·8, 81°·3.

† Equivalent respectively to 79°·16, 79°·7, 80°·06, and 80°·24 of Fahrenheit's scale.

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the entire oceanic covering of our planet, compared to its atmospheric coating.

In comparing the calorific influence of the land on distant regions with the agency of the sea, it should therefore be remembered that, while the latter stores up heat, and acts by night as well as by day, the action of the land is effective only as long as the sun's rays are impinging upon it.

4. Let us endeavour to apply these conclusions to the question of the influence of the distribution of land and water upon general terrestrial temperature. As the amount of solar heat received by any point on the earth's surface is a function of the latitude, it follows that the distribution of land and water at different latitudes must be studied in order to obtain its influence on temperature. This distribution may be supposed to take place in an endless variety of ways, of which the following three cases are the most important:—

1. Preponderance of land towards the poles, and of water towards the equator. 2. Preponderance of land towards the equator, and of water towards the poles. 3. Equable distribution of land and water in polar and equatorial regions.

At the present day three-fourths of the earth's surface are covered with water, so that all the dry land has been truly characterized as an assemblage of large and small islands placed in a great ocean. If we suppose, with Sir Charles Lyell*, that, in the question now under consideration, the proportion of sea to land is the same as at present, each of the above three cases is susceptible of two principal divisions, according as the islands composing the land happen to be few and large, or numerous and small. If all the dry land on the globe were collected into a single vast continent, the climatological conditions of the earth, all other things remaining the same, would be very different from what would take place if the land were broken up and spread out in numberless islands. Whatever may be the supposed distribution of land and water, it is manifest that its chief influence on the general temperature at the surface of our planet would result from the action of aërial and oceanic currents.

In the first case above referred to, the belt of equatorial ocean would probably acquire a high temperature; and although the circumpolar islands would possess very rigorous climates in their interior, portions of their coasts might be washed by heat-bearing currents, just as the north-western coast of Europe is washed by the Gulf-stream at the present day. The superiority of mean temperature of the ocean might in this case be so great that the distribution of heat over the islands would present remarkable

* Principles of Geology, chap. vii. 9th ed. p. 101.

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instances of the laws found to hold good in the British Isles, and almost all of the isothermals on the land would be closed curves*.

In the second case, the ocean would acquire much less heat from the sun, and it would exercise a cooling influence on the belt of intertropical land. But, as whatever evidence we possess seems to indicate that intertropical seas owe their elevated temperature not so much to the influence of thermal exchanges with the air which has passed over the adjacent land as to the direct influence of sunshine, we may conclude that, upon the whole, the heat-bearing currents would in this case be less influential than in that which has been just considered. The heated air flowing from the equatorial land should, by the agency of winds, in some measure mitigate the temperature of the polar regions; but we have no reason for believing that this influence would be superior to that of the heat-bearing water-currents in our former instance.

If, now, we suppose the land to be equally distributed in islands between the equatorial and polar regions, we shall have conditions more or less favourable to the existence of oceanic as well as of aërial heat-bearing currents; and it seems not impossible that under such circumstances the entire surface of the globe might enjoy the highest possible amount of general warmth, by being best circumstanced for the accumulation, retention, and distribution of the heat it receives from the sun. In this case, as well as in the first which has been considered, warm currents from the equatorial seas might freely bathe the coasts of islands in higher latitudes, thus producing similar characteristic cases of insular climate. The mean temperature of such seas being higher than that of the air over the land, the isothermal lines of the islands should be partly or entirely closed curves, having shapes dependent upon the outlines of the islands. The greater the difference of atmospheric and water temperature, the more strictly should the isothermals conform to this law. Thus, it is manifest that a nearly circular island with a surface equal to that of Labrador, and lying in the same latitude, would present a much greater diversity of climate between its interior and its coasts, if the latter were bathed by sea-water having a temperature of 80° Fahrenheit, than if that temperature amounted only to 40°. As the manner in which the warm air over the water would exchange its heat with the air over the land would undoubtedly be by circulation, it would not be easy to assign a distinct law for the difference of temperature between the interior and the coast of the island; but it seems evident that this difference should, up to a certain limit,

* See Phil. Mag. for October 1858, p. 249.

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increase with the temperature of the heat-bearing oceanic currents. A group of islands situated in high latitudes, and surrounded by currents possessing a high temperature, while receiving but a small amount of heat from sunshine, should present a series of closed isothermals, and, while their interiors would be cold, their coasts might enjoy an extremely genial climate.

5. If such conditions existed at former geological epochs, we may fairly expect to find some evidence of their existence by comparing the characters of the organized beings by which the interior and the coasts of such islands were inhabited. Such geologists as have hitherto studied the diversities in structure of the fossil remains which have come under their notice, appear to have attended principally to the climatic influence of the elevation of the interior parts of such islands. Professor Ramsay*, in his memoir on the denudation of Wales, after pointing out the great elevation above the sea, which portions of that region had formerly possessed, calls attention to the resulting varieties of climate that must have prevailed. "If," he says, "the climate of our latitudes, when the coasts were washed by the new red, and liassic seas, were tropical, as is generally supposed, still, on the heights indicated on the vertical sections, we have ample space for tropical and temperate zones, each probably abounding in its own appropriate forms of life. And here, in connection with this subject, it may be remarked that in Mr. Brodie's recent work, 'A History of the Fossil Insects of the Secondary Rocks of England,' it has been stated that, with certain exceptions, the minute size of the great mass of the insect remains seems to indicate a very cold, or at all events a temperate climate."

This appeared to Professor Ramsay not to be in harmony with the other fossil evidence, which proves that most of the creatures whose remains are preserved in the strata of the secondary series inhabited a tropical climate. If the interior temperature of the land, whose inhabitants apparently existed under such different conditions of climate, depended not only on the coordinate of height above the sea, but also on that of distance from the coast in the manner here described, a more complete explanation would be afforded of these remarkable phænomena. The discovery by Mr. Strickland, in the alluvial sand of Worcestershire, of the bones of a hippopotamus, accompanied not only by the bones of other mammalia, but by twenty-three species of fresh-water and land shells, of which nineteen are existing British species, seems to show that, even at a period so recent as that of the deposit from which these remains were taken, remarkable differences of climate may have existed over a comparatively

* Memoirs of the Geological Survey of Great Britain, vol. i. p. 324.

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small area of land*. The strong presumptions furnished by the fossil flora, and other evidences connected with the history of earlier geological formations, in favour of the existence of numerous islands scattered over an ocean enjoying a tropical temperature, should lead us to expect more of such results as are here noticed, instead of feeling surprise at the discrepancies which they seem to exhibit.

6. I shall now attempt to illustrate some of the preceding general views from the actual condition of the earth's surface. The higher mean temperature of the northern compared to the southern hemisphere is clearly proved and universally acknowledged. This superior warmth is usually ascribed to the greater amount of land in the former compared with the latter. It has been apparently assumed that the surface of the dry land exercises upon the whole a far more energetic influence, in tending to elevate the mean temperature of the earth, than the surface of the water; and this action is generally ascribed to the superior heat-absorbing power of land compared with water. Upon this assumption is mainly founded the beautiful and elaborate theory of geological climates, which Sir Charles Lyell first published in his 'Principles of Geology.' Although Fourier had previously indicated the possible influences exercised upon terrestrial temperature by the physical conditions of the earth's outer coating, he had not given his views such a definite shape as to enable him to deduce any conclusions from them for the solution of the great problems of terrestrial physics which have so much occupied the attention of philosophical geologists.

If the conclusions of the theory now referred to be correct, it follows that predominance of land over water between the tropics, where an absorbing surface would be most advantageously circumstanced for acquiring heat, should result in producing the highest possible degree of general terrestrial temperature. On the contrary, the earth's general climate would be reduced to a maximum of coldness by a predominance of land towards the polar regions, and of water towards the equator. The views developed in this essay would appear to require some modification in these conclusions; and the first especially is not in perfect harmony with the results to which we have been led by such reasonings as I have here presented. Not only are there physical grounds for adopting a somewhat different conclusion (namely, that the most favourable condition for a generally high terrestrial temperature would be in a comparatively equable distribution of land and water over equatorial and extratropical

* Geological Society's Proceedings, June 1834, p. 94; and Lyell, p. 76, 9th edition.

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regions, instead of a concentration of land in the former), but the study of the present relations of sea and land seems to strongly verify the views on which this conclusion is based.

If we look over a terrestrial globe, or a good stereographic projection of its surface*, we soon perceive that in the regions traversed by the ecliptic, and where, consequently, the sun's rays diffuse the greatest amount of heat over absorbing substances, land and water are distributed very evenly at both sides of the equator. Each hemisphere absorbs the greatest quantity of solar heat during the six months when the sun is vertical over some part of its surface; and I have found that the parallel of 7° 24′ receives the maximum amount of sunshine during the summer half year. In the northern hemisphere this parallel runs from the coast of Guinea through central Africa; crossing the Indian Ocean, south of Cape Comorin, it passes through Ceylon across Malacca and the island of Mindano; thence through the Pacific, until it meets South America, the northern portion of which it traverses from a point near the Gulf of Panama to another between the mouths of the Orinoco and Esiquibo. In the opposite hemisphere, the parallel of maximum southern sunshine crosses Africa from a point north of St. Paolo de Loando to another near the Monfeca islands. It traverses a great part of Java, New Guinea, and smaller islands. It crosses South America almost on the line of greatest breadth, from near Truxillo to a point north of Pernambuco. On comparing the extent of land and water lying under the parallel of maximum half-yearly sunshine, it appears that the proportions are nearly the same in both hemispheres, although a very slight excess of land appears to lie under the southern, compared to the northern parallel†. Outside the torrid zone, the proportions of land and water belonging to each hemisphere respectively are extremely different: while nearly half of the surface between the pole and the tropic of Cancer is land, by far the greater portion of the area between the southern tropic and the pole is water. In the arctic and antarctic regions land and water alternate in nearly corresponding proportions. The great difference between the areas of land and water of the northern and southern hemispheres exists in the temperate regions. Upon the whole, it may be concluded that there is a comparative predominance of land over water in the higher latitudes of the northern hemisphere, while the opposite condition holds in the southern hemi-

* M. Babinet's homolographic maps are still better adapted for such comparisons as that now made. See Arago, *Astronomie*, tome iii. p. 344; Report of the British Association for 1856, Trans. Sections, p. 112.

† See the preceding Article (*Atlantis*, No. 3. p. 201), on the laws which regulate the distribution of isothermal lines, § 5.

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sphere. If the presence of dry land in high latitudes is favourable to a cold climate, this condition appears to be more completely manifested in the northern than in the southern hemisphere; and if the presence of a certain amount of dry land within the tropics is favourable to a high temperature, that condition is almost equally well fulfilled at both sides of the equator.

Let us conceive all the land north of the equator to be submerged, and its place to be supplied, first, by a mass of land in the north tropical zone, exactly similar in area and configuration to that touching it in the southern zone. Let the arctic regions of North America, Nova Zembla, and Greenland be replaced by an island similar to Victoria Land, and let a few scattered islands replace the greater part of Asia, Europe, and North America; we shall then have a globe with a considerable belt of equatorial land, while the polar and temperate regions will be occupied chiefly by water. We should thus have a state of things approximating much more to the conditions required for a high terrestrial temperature than the present distribution of land and water. Yet the distribution here supposed for both hemispheres would be precisely what at present exists in the colder of the two; and we should thus have the paradox of warming the entire globe by modelling its warmer hemisphere after its colder. Unless the influence of Victoria Land as a refrigerator of the southern hemisphere should be greater than that of the immense masses of land in the northern parts of the new and old continents, this paradox would seem inexplicable on the theory under consideration. But it can be in some measure explained, if the agency of oceanic currents in storing up and transporting the heat acquired from sunshine be fully admitted. In the actual state of the earth's surface, the form of the basin of the South Atlantic Ocean, combined with other physical conditions, seems to determine the transfer of a great volume of heated water from the southern intertropical regions to the northern hemisphere, which, passing subsequently through the Caribbean Sea and Gulf of Mexico, acquires a still higher temperature, and ultimately confers its warmth on regions in high northern latitudes. From the direction of the currents of the Pacific, as laid down on some of Maury's charts, it is probable that a similar transfer northwards, of heated southern intertropical water, is effected in that great ocean as well as in the Atlantic. The general result is, that the southern hemisphere is not only deprived of a certain amount of the solar heat absorbed by its waters, but that the temperature of the northern hemisphere is augmented to a corresponding amount. But although the paradox alluded to may be thus explained, this

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result shows the danger of under-estimating the agency of aqueous currents in connexion with any theory of the distribution of land and water that may be proposed in order to explain vicissitudes of terrestrial climate.

7. In examining the consequences, resulting from the suppression of the Gulf-stream, on the climate of western Europe, with reference to the question of glacial action at former geological epochs, as has been done by Mr. Hopkins*, we need only direct our attention to what actually takes place at corresponding latitudes in the southern hemisphere. In these regions, there is not only an absence of such an active calorific agent, but even an abstraction of some of the heat due to them from the sunshine which falls upon a portion of their oceans, which heat we have seen is transferred to the northern hemisphere. Glaciers consequently descend to the sea, not only about the latitude of 54° S., as observed by Captain Cook, but even so close to the equator as 48° 30′ S., where they were noticed in great abundance on the western coast of South America by Mr. Darwin†. He even observed one instance of a glacier reaching the sea in the latitude of 46° 40′, which corresponds to that of Napoléon Vendée, in the west of France. The existence of glacial action in the southern latitudes, equivalent to those of the temperate regions of western Europe, suggests the possibility that, by an inversion of the operating causes, the southern hemisphere might have enjoyed a milder climate at the same geological period that glacial phænomena were most completely developed north of the equator.

8. The results of our inquiry may be thus recapitulated:—

(1) The physical properties of water appear upon the whole more favourable than those of the land, to the accumulation, retention, and distribution of solar heat throughout the matter composing the external coating of the earth.

(2) Phænomena presented by intertropical seas at the present day, confirm and illustrate this conclusion.

(3) The distribution of land and water most favourable to a general increase of terrestrial mean temperature, should, therefore, be such as would imply the existence of great intertropical seas and of groups of islands evenly distributed both within the tropics and in extratropical regions.

(4) Such a distribution of land and water at former geological epochs, seems to be indicated by the results of observation.

(5) The superior mean temperature of the northern compared to the southern hemisphere is probably due, not to the direct in-

* Quarterly Journal of the Geological Society, 1852, p. 85.

† Voyage of 'Adventure' and 'Beagle,' iii. p. 282.

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fluence of the greater proportion of land in the former, but to currents which determine the transfer towards the north of a portion of the solar heat absorbed south of the equator.

9. While fully acknowledging the important influence which changes in the distribution of land and water may exercise on terrestrial climate, we are not precluded from studying the action of other causes, and of giving to them such weight as the evidences in their favour may render advisable. If, from the results of astronomical as well as of geological testimony, we are induced to believe that the earth has been for ages slowly cooling from a state of former incandescence, its climate during the earlier epochs of its physical history must have been more or less influenced by the heat thus passing outwards through its crust. However efficient, as applied to recent phænomena, we may find the theory of geological climates that explains the variations of the earth's superficial temperature by changes in the distribution of the liquid and solid portions of its outward coating, it seems by itself incompetent to rationally and consistently account for the very high temperature which must have prevailed during remote epochs of the earth's history. If we reject the evidence on which it has been concluded that the earth has slowly cooled from a fluid incandescent state into its observed condition, and admit that the earth's spheroidal shape was due to gradual and even existing causes, and not to the mechanical consequences of its primitive and universal fluidity, we shall arrive at a conclusion which, on the supposition of the complete adequacy of superficial causes to explain all changes of climate, would lead to the inference that, from very remote epochs, the mean temperature of the globe should be increasing instead of diminishing. By rejecting the former fluid condition of the earth, we are compelled to account for its oblateness in the way attempted by Playfair, that is, by appealing to the influence of certain superficial actions coexisting with the phænomena of geological changes. But I have proved * that if, from superficial causes, the earth's figure became gradually more oblate, the extent of polar dry land would gradually diminish, while that of equatorial dry land would at the same time tend to augment. Hence the very operations required to mould the earth's figure into the shape now observed, would, on this theory, point to a gradual increase in the efficiency of the physical conditions required for an augmentation of terrestrial temperature in proceeding from the most remote to the most recent geological epochs. But this is the very reverse of the conclusions deduced

* Proc. Royal Irish Academy, vol. iv. p. 333; and Journal of the Geological Society of Dublin, March, 1849.

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from the entire mass of geological inquiries; hence, as far as observation enables us to judge, we cannot explain by superficial actions alone the twofold condition—of the spheroidal shape in the earth's figure, and the gradual diminution of its surface temperature from the earliest periods of geological history down to the most recent.

XXIX. *On a Compound of Dibromallylammonia and Chloride of Mercury.* By Dr. Maxwell Simpson*.

In a paper published in this Journal (October 1858) on the Action of Ammonia on the Terbromide of Allyle, I mentioned that dibromallylammonia forms a compound with chloride of mercury. This compound I have since studied; the following are the results.

The body in question makes its appearance in the form of a white voluminous precipitate, on mixing alcoholic solutions of dibromallylammonia and of chloride of mercury, keeping the latter in excess. The excess of chloride can be afterwards removed by washing the compound with water, in which it is almost insoluble. The body thus prepared was submitted to analysis, having been previously well dried *in vacuo* over sulphuric acid.

The following numbers were obtained:—

I. 0·4130 grm. compound gave 0·2138 grm. carbonic acid and 0·0781 water.

II. 0·5052 grm. compound gave 0·2268 grm. sulphuret of mercury.

III. 0·2690 grm. compound gave 0·1562 grm. chloride of silver†.

IV. 0·4446 grm. compound, heated with quicklime, gave 0·5584 grm. mixed chloride and bromide of silver; and 0·5123 grm. of this mixture lost 0·0676 grm. on passing chlorine over it.

These numbers agree with the formula $2(HgCl)+N\left\{\begin{matrix}C^6H^4Br\\C^6H^4Br,\\H\end{matrix}\right.$ or perhaps $HgCl+N\left\{\begin{matrix}C^6H^4Br\\C^6H^4Br\\Hg\end{matrix}\right\}HCl$, as will be seen from the following per-centage Table:—

* Communicated by the Author.

† This determination was made by dissolving the compound in dilute nitric acid and adding nitrate of silver. The chlorine was alone precipitated, the bromine remaining in solution.

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	Theory.		I.	II.	III.	IV.
C^{12}	72	13·69	14·12			
H^9	9	1·72	2·10			
N	14	2·66				
Hg^2	200	38·02	...	38·70		
Cl^2	71	13·50	...	...	14·36	13·63
Br^2	160	30·41	...	...	...	29·53
	526	100·00				

This body is very sparingly soluble in cold water; boiling water decomposes it with the formation of a purple-coloured compound. It is freely soluble in alcohol, from which it crystallizes in long needles. It dissolves also in dilute nitric and hydrochloric acids. The solution in the latter acid gives, on the addition of potash, a white precipitate, which gradually becomes yellow.

XXX. *On the Action of Chloride of Acetyle on Aldehyde.* By Dr. Maxwell Simpson*.

Bertagnini having succeeded in forming cinnamic acid by exposing oil of bitter almonds to the action of chloride of acetyle, it occurred to me that possibly the action of the same body on ordinary aldehyde might give birth to the acid $C^8 H^6 O^4$†, intermediate, in the acrylic series, between acrylic and angelic acids. The following equation will explain how it might be formed:—

$$C^4 H^4 O^2 + C^4 H^3 O^2 Cl = C^8 H^6 O^4 + HCl.$$

In order to determine this point, I exposed a mixture of equivalent quantities of chloride of acetyle and aldehyde in sealed tubes to the temperature of a water-bath for about three hours. At the expiration of this time I removed them from the bath, and opened them as soon as they had become cold. No evolution of gas could be observed. The liquid, submitted to distillation, did not commence to boil till it had reached the temperature of 90° C.; and between that temperature and 140° almost the entire quantity passed over. On fractioning it, a considerable quantity was obtained, distilling between 120° and 124°, which was reserved for analysis. The following are the results:—

I. 0·2228 grm. of liquid gave 0·3158 grm. carbonic acid and 0·1139 water.

* Communicated by the Author.

† Crotonic acid is supposed to have this composition.

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II. 0·2658 grm. of liquid gave 0·3796 grm. carbonic acid and 0·1382 water.

III. 0·2699 grm. of liquid gave 0·3054 grm. chloride of silver.

These numbers lead to the formula $C^8 H^7 O^4 Cl$, as will be seen from the following per-centage Table:—

	Theory.	Experiment. I.	II.	III.
C^8 . .	39·18	38·65	38·95	
H^7 . .	5·71	5·68	5·77	
O^4 . .	26·14			
Cl . .	28·97	...	...	28·00
	100·00			

There is then in this case a simple coalescing of the two molecules, without the separation of hydrochloric acid. This liquid is heavier than water, which decomposes it with extreme slowness when cold, but rapidly when hot. It dissolves readily in dilute potash,—chloride of potassium and acetate of potash being formed, and aldehyde regenerated, which is converted into a resin by the action of the alkali when the latter is in excess. If, however, the liquid be cautiously neutralized, almost all the aldehyde is given off as vapour, a mere trace being resinified. In order to be certain of the formation of acetate of potash in this reaction, I evaporated the solution in potash to dryness on a water-bath, and treated the dry mass with absolute alcohol, which dissolved the acetate, leaving the chloride undissolved. The acetate was then converted into the silver-salt and analysed.

The following numbers were obtained:—

0·2087 grm. of silver-salt gave 0·1346 grm. silver.

Per-centage composition:—

	Theory.	Experiment.
Silver . . .	64·67	64·50

Moist oxide of silver reacts on this liquid in the same manner, chloride and acetate of silver being formed.

The body which I have just described has already been discovered by M. Wurtz amongst the products of the action of chlorine on aldehyde. He supposed that its formation was due to the coalescing of two molecules of aldehyde, and to the subsequent action of the chlorine on this polymeric modification. The foregoing, however, proves that the body in question is simply formed by the direct combination of the chloride of acetyle (the chief product of the action of chlorine on aldehyde) with some unaltered aldehyde.

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XXXI. *Remarks on a paper "On Ice and Glaciers" in the last Number of the Philosophical Magazine. In a letter to* Prof. Tyndall. *By* Prof. J. D. Forbes.

My dear Sir,

In the part of your short paper "On Ice and Glaciers," in the Philosophical Magazine for February, in which my writings are referred to, I think that my meaning has been misapprehended. Perhaps I can very briefly state what, in each of two cases, I conceive to be a needless source of difficulty unintentionally entertained by you with reference to my theoretical views or explanations.

The explanations which I now offer will tend, I hope, towards a reconcilement of our conclusions, and not to controversy.

I. With reference to the theory of "Regelation," you say, on p. 93 of the Phil. Mag. for February,—"Why is the 'rapid pounding' necessary in the experiment of Prof. Forbes? Doubtless in order that the ice may be brought into contact with the thermometer *before* its temperature has risen to 32°. But give the ice time to rise to 32°; let its last residue of cold be abolished—the mass thus warmed, and in which the finest thermometer will not show the smallest fraction of a degree below 32°, may, with the utmost facility, be converted by pressure into *solid ice*."

And again, "Let the thawing surface of a mass of ice be scraped away, so as to obtain a fine ice-powder possessing the temperature of that surface. Let not the alleged magazine of cold *within the ice* be at all called upon; such a powder, or more properly fine slush, the temperature of which no thermometer can show to be below 32°, may, as in the former case, be converted by pressure into solid slabs of ice."

According to my view, in the former of these cases the thermometer can only show the temperature of the *ice* when the latter is freshly pounded. If kept at a thawing temperature, each icy fragment becomes invested with a film of water which has the temperature of *water*, not of *ice*. You say, "Give the ice time to rise to 32°." I answer, that it is not in the nature of ice to do so. The heat communicated becomes latent in the water produced, not one iota of it reaching the ice, which, whilst it remains Ice, is incapable of having its temperature raised. Precisely the same consideration applies to the second paragraph. Granulate the ice as we will, whilst any Ice remains, it has the temperature of Ice, not of Water. The powder or fine slush which you describe has the temperature of 32°, because the thermometer is in contact with the water alone, which forms so large a part of its composition. A thermometer is, in short, incapable

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of taking the temperature of ice unless that ice be *dry*. What, then, is the effect of the pressure which you describe as converting the "slush" into "solid slabs"? You admit, with me, that pressure is not necessary in the general case for regelation (p. 94). The effect of the pressure, then, is merely to banish nearly all the perfect water (that whose temperature the thermometer had shown) from the mass, leaving merely films of water (more or less viscid) between the still icy particles which constituted the slush. These particles, small as they are, contain each its magazine of cold, and being numerous as they are small, this cold is adequate to the consolidation of the interposed films of water, precisely as when the experiment is made on larger masses.

In this explanation I admit your view, that water is absolutely frozen in the process; but I believe also in the cohesive aggregation or *welding* under pressure of surfaces of ice softened by imminent thaw, though not yet reduced to water.

I think that, on consideration, it will appear clear to you that the preceding explanations render the facts you mention inevitable deductions from the doctrine that ice absorbs latent heat *gradually**.

II. As regards the theory of the Veined Structure of Glaciers (p. 94 of your paper), there would perhaps be some inconvenience in my dictating the interpretation which certain passages of my writings more than twelve years old must bear, and declaring that no other interpretation is admissible. Such an *affidavit* of a pleader in his own cause might be received with distrust. It is better that others should study what I have written, and that they should conclude my meaning from an impartial examination of *the whole* documents. To facilitate this examination, I am about to publish, in a collected form, all my minor papers on the Theory of Glaciers which can throw any light on this question; and I hope that *then*, when the difficulty of fairly and fully examining what I have said on the subject has been removed, my meaning will be found to be both definite and intelligible.

In the present instance, if, as stated in your paper, more than one writer has attached a certain significance to the terms of my Thirteenth Letter on Glaciers—and if, of these writers, Professor William Thomson be one,—it is not, I think, too much to affirm that it is probable that their concurring, though independent, inference as to my meaning is correct. I have not myself any doubt of its correctness; but I repeat that I do not cite myself as a witness in a case of interpretation which affects my own

[* See Mr. Faraday's remarks on Regelation in our present Number, p. 162.—Ed.]

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credit. Professor Thomson, or some one who maintains the same opinion which he does, will, I have no doubt, be ready and willing to give grounds for his judgement.

Why I very reluctantly appear in the matter is for this reason —that I believe you have not altogether understood, perhaps have overrated, what I claim to have established, particularly in connexion with the change admitted by all parties to have occurred in some of my ideas, in consequence of my journey in 1846, described in the Thirteenth Letter before mentioned. Several passages at pages 95 and 96 of your paper seem definitely to indicate that you ascribe to me an abandonment of a previously elaborate theory of the cause of the Veined Structure, and the substitution of a theory subversive of, and superseding in all points the old one. I will cite only one passage, including a quotation from myself, as given at p. 95 of your paper. It is as follows:—

"'Mr. Hopkins,' writes Professor Forbes in 1845, in reference to an experimental proof, 'denies that the ribboned (veined) structure is produced by differential motion . . . . . *No person who has seen the model made, or even been told how it was made, and inspects the ribboned structure upon its surface, can, I think, unless influenced by previous theoretical views, entertain any other opinion.*' Is it to be supposed that convictions thus strongly uttered, based upon years of observation, and established, according to the above quotation, by the testimony of the senses themselves, are meant to be reversed by a single observation, which, after all, is essentially defective, involving, in reality, not a *fact*, but an *opinion*?"

The italics are yours. Here I am represented as stating a theory of the veined structure in 1845, which, it is argued, I could not (as I am represented to have done) have relinquished in merely one or two sentences of a letter of 1846. I shall probably give you satisfaction when I assure you that the doctrine of the origin of the veined structure, so far as described in the preceding citation, and in several other passages of those pages of your article already referred to (pp. 95 and 96), was neither given up by me in 1846, nor in any subsequent year—nay, that I hold it still. I also entertain the very same views with reference to Mr. Darwin's theory of banded lavas which I did in 1845, which at page 96 you understand me to have abandoned, or at least that I am represented to have abandoned them, in 1846*.

[* For the sake of clearness, I hope I may be permitted to state that the assumption to which I referred is expressed by Mr. Darwin in the following words:—"In the ice the porous laminæ are rendered distinct by the subsequent congelation of infiltrated water;" and my statement is, that

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The difficulty, which until 1846 did not become thoroughly plain to me, was this:—How is the structure of glassy ice induced in the glacier without previous fusion and fresh congelation? In 1842 I believed that the plates of hard blue ice, which by their interposition between spaces of a less compact character, compose the Veined Structure, could only be due to the freezing of water in the glacier. I considered them as veins of infiltrated water frozen during winter. To the same cause I then attributed the conversion of the granular mass of the *névé* into glassy ice. It was always a reluctant admission on my part: for the congelation of infiltrated water to any great extent, even in winter, was a relic of the Dilatation theory of glacier motion; and against that theory I had entered an earnest protest. Between the printing of my Fourth Letter on Glaciers in 1842, and the publication of my 'Travels' in 1843, I had already abandoned the use of this reluctantly admitted fact—the winter congelation of the *depths* of the glacier—so far as I had employed it to explain the recovery of the level of the glacier between autumn and the following spring; and I felt my position to be stronger when I obtained another explanation *. A similar but more effectual deliverance from a felt difficulty arose in 1846, when I arrived at a clear persuasion of what (as will appear from a comparative study of my writings) had already dawned upon me some time previously, namely, that the glassy structure of ice is attainable by the cohesion under pressure (especially if aided by motion with friction, or *kneading*) of the semi-opake and porous material of the glacier.

Although I had previously expressed several times my growing belief that the renewed cohesion of the bruised surfaces of the *ice* in the glacier proper, under the mutual pressure of their parts, might account for the facts both of motion and of structure there, I had not until the summer of 1846 disembarrassed myself of the complication arising from the conversion of granular snow in the *névé* into pellucid ice. I had, indeed, come very near to it; for already, in 1844, I had approximated the two phænomena in the following passage, in which, after speaking of the differential motion which tends to produce the veined structure in the glacier proper, I added—"I believe that it is

Prof. Forbes does not use a word which would lead us to suppose that he wished to modify that assumption.—Phil. Mag. 1845, vol. xxvi. p. 354.—J. T.]

* 'Travels,' 1st ed. (1843), p. 384, or 2nd ed. p. 386; and note the limitations under which congelation in the interior of the glacier is admitted, at pages 232, 360, 372 of both editions. In so far as the *glacification* of the *névé* is concerned, it was no assumption of mine. I accepted the universal opinion of the time as stated by De Saussure, as well as by MM. de Charpentier and Agassiz.

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during the progress of the glacier thus subjected to a new and peculiar set of forces depending upon gravity, and which remodel its internal constitution by substituting hard blue ice in the form of veins for its previous snowy texture, that the horizontal stratification observed in the higher part of the glacier or *névé* is gradually obliterated*." You see that even at this date the formation of the hard blue ice of the veined structure, and the conversion of the *névé* into pellucid ice, appeared to me to be very intimately connected,—so much so, that until the last could be explained without interior congelation, little was gained in point of simplification by rejecting it in the former instance. It will be found, as I have said, by a consecutive perusal of what I wrote between 1844 and 1846, how gradually I approached the conviction (only obtained on the spot in the latter year) that the phænomena might be *all* explained by pressure and cohesion, the latter arising from the softening of snow or ice when thaw is imminent or in progress. In 1846, then, I abandoned no part of the theory of the veined structure on which, as you say, so much labour had been expended, except the admission, always yielded with reluctance, and got rid of with satisfaction, that the congelation of water in the crevices of the glacier may extend in winter to a great depth†.

I remain, dear Sir,

Prof. Tyndall,
Royal Institution.

Yours faithfully,
JAMES D. FORBES.

P.S.—It is foreign to the purpose of this letter to express an opinion as to whether any theory which you have formed as to the "veined structure" is independent, or otherwise, of the fundamental idea of differential motion arising under conditions of unequal pressures combined with friction. I reserve it until your matured views are published. But it did appear to me, some time ago, that in Mr. Sorby's most ingenious researches on slaty cleavage, and in your extension of them to homogeneous bodies such as wax and dough, the efficiency of differential motion was tolerably apparent.—J. D. F.

* Sixth Letter on Glaciers, 1844.

† That this is a question of some nicety will be readily perceived; and my caution in overtly rejecting its influence altogether may be understood. For winter congelation must act to *some* depth; and certain phænomena cannot be explained without it. Such are the lenticular frozen cavities which you have described in your earliest paper on glaciers, which I take to be the same as those longitudinal infiltrated cracks previously described by me as visible in the glacier of Bossons, and other torrential glaciers. Such icy infiltrations are apparently too wide to be produced except by direct congelation. But then they are always near the side or surface of a glacier.

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XXXII. *A Mathematical Theory of Heat.* By Professor CHALLIS*.

IT is a fact of experience, that when rays of light pass through the earth's atmosphere, or other substance, its temperature is increased. It would seem, therefore, that if we knew exactly what rays of light are, we should have some clue for determining the agency by which heat is produced. In the following outline of a mathematical theory of heat, I adopt the hypothesis that light-bearing undulations are such as were defined in my communication to the Philosophical Magazine for February, and that, while light is due to the transverse vibrations, heat is the result of the mechanical action of the direct vibrations. The heat-producing undulations are supposed to be compounded, as stated in the same communication, of simple wave-rays, in such a manner that the transverse vibrations destroy each other.

The ultimate atoms of material substances are *inert*, because the inertia of a mass is the result of the inertia of its constituent parts; they have *magnitude* and *form*, otherwise they are not within the compass of mathematical reasoning; their form is that of a *sphere*, because they are found by experience to have individually the same relations to all parts of space; and they are *hard* and *impenetrable*, otherwise the spherical form is not necessarily preserved. These hypotheses, which, with the exception of that of the spherical form, agree with the views expressed by Newton respecting the ultimate properties of matter (Optics, Book III. Qu. 31), will be adopted in the following reasoning. If the constituent atoms of bodies have other properties, it will be time to inquire what they are when these are found to be insufficient. Further, it will be assumed that, in a substance of uniform density, the constituent atoms are distributed uniformly in space, and, in conformity with an inference drawn at the end of the communication above referred to, that the space occupied by the atoms, even in the case of solid bodies, is extremely small compared to the intervening space, so that the radius of an atom is extremely small compared to the mean distance between neighbouring atoms.

If, now, a series of plane-waves of alternate condensation and rarefaction traverse any medium, just as light-bearing waves are known to traverse transparent substances, each atom of the medium becomes a centre of secondary waves, in a manner analogous to the production of such waves when a small obstacle is encountered by waves propagated on the surface of water. According to the hypothesis with which we set out, the caloric repulsion of an atom is due to the agency of these subordinate

* Communicated by the Author.

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waves. It may be supposed that an unlimited number of such waves are produced by series of plane-waves traversing the medium in all directions, and that, when an equilibrium of heat is established, these reflected waves are propagated equally in all directions from an atom, or the resulting velocity and condensation are functions of the distance from the atom. To determine the dynamical action of waves so propagated from a centre, is a hydrodynamical problem, which I now proceed to consider.

The article in the Number of the Philosophical Magazine for February 1853, contains, under Prop. XIII., an investigation of the differential equations to the first approximation applicable to motion propagated equally in all directions from a centre. The exact equations obtained on the same principles are,

$$\frac{\kappa^2 a^2 d\rho}{\rho\, dr} + \frac{dV}{dt} + V\frac{dV}{dr} = 0, \quad \ldots\ldots \quad (1)$$

$$\frac{d\rho}{dt} + \frac{d.V\rho}{dr} + \frac{2V\rho}{r} = 0, \quad \ldots\ldots \quad (2)$$

the first of which differs from the equation usually obtained for this problem by having κa in the place of a, the value of κ being 1·18545. (See the Article "On the Central Motion of an Elastic Fluid" in the Philosophical Magazine for last January.)

The known integrals of these equations applicable to propagation from a centre are, to the first approximation,

$$V = \frac{f'(r-\kappa at+c)}{r} - \frac{f(r-\kappa at+c)}{r^2},$$

$$\kappa a\sigma = \frac{f'(r-\kappa at+c)}{r}.$$

Respecting the function f it is to be observed that its form is not entirely arbitrary, because the secondary waves under consideration have their origin in waves whose velocity and condensation are expressed by periodic functions. It is evident, in fact, that V must have as many *plus* as *minus* values, since there can be no permanent motion of translation of the fluid to or from the centre. Hence we may assume as follows:—

$$\frac{f(r-\kappa at+c)}{r^2} = \frac{1}{r^2}.\Sigma\left[m \sin\frac{2\pi}{\lambda}(r-\kappa at+c)\right],$$

$$\frac{f'(r-\kappa at+c)}{r} = \frac{1}{r^2}.\Sigma\left[\frac{2\pi m r}{\lambda}\cos\frac{2\pi}{\lambda}(r-\kappa at+c)\right].$$

As the ratio of the maximum values of corresponding terms on the right-hand sides of these equations is $\frac{2\pi r}{\lambda}$, it follows that the first term in the value of V is much less than the other for

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all values of r that are very small compared to λ. If therefore such values be alone considered, the first term, and consequently the condensation σ, may be neglected; and since the velocity of propagation in the ætherial medium is extremely great, and may be regarded in this problem as infinite, we shall have very approximately the same value of V as if the fluid were incompressible, viz. $V=\frac{f(t)}{r^2}$.

In order to include terms involving the *second* power of the velocity, the equation (1) is to be integrated, retaining the last term. We thus obtain

$$\kappa^2 a^2 \,\mathrm{Nap.log}\frac{\rho}{\rho_1}+\int\frac{dV}{dt}\,dr+\frac{V^2}{2}=0;$$

and if $\rho=\rho_1(1+\sigma)$, to the second power of σ

$$\kappa^2 a^2\left(\sigma-\frac{\sigma^2}{2}\right)+\int\frac{dV}{dt}\,dr+\frac{V^2}{2}=0.$$

This equation gives the condensation σ at any distance r from the centre of the atom from which the waves are supposed to be propagated, whence the pressure of the fluid on another atom situated at a given distance from the first might be inferred. But so far as the condensation is periodic, it will only give rise to *oscillatory* motions of the second atom, and may therefore in the present inquiry be left out of consideration. Hence since V, as we have seen, is necessarily periodic, and the above term affected with the sign of integration is consequently periodic, this term may be omitted. Also it will suffice to substitute for V in the last term the value given by the first approximation. Hence since

$$V=\kappa a\sigma-\frac{1}{r^2}.\Sigma\left[m\sin\frac{2\pi}{\lambda}(r-\kappa at+c)\right],$$

by substitution in the foregoing equation and neglecting periodic terms, we shall finally obtain

$$\kappa^2 a^2\sigma+\frac{\Sigma[m^2]}{2r^4}=0.$$

This result shows that the part of the condensation which has the effect of giving a permanent motion to the atom, varies inversely as the *fourth* power of the distance, and increases with the distance, since σ is negative; so that the second atom is by this pressure urged *towards* the first atom. But as the differential pressure urging it varies directly as its radius, and inversely as the *fifth* power of the distance, the accelerative force from this cause must be extremely small, and not comparable with other forces that have to be taken into consideration.

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Let us now investigate the effect produced by the impinging of the part of the velocity expressed above by $\frac{f(t)}{r^2}$ on any atom, supposing this velocity to be unaccompanied by condensation. The function $f(t)$ must be periodic, and, taking account of its origin, may be put under the form $\Sigma[m \sin(bt+c)]$. It may be supposed that the atom on which this velocity impinges is at rest, because when an equilibrium of caloric action is established, the motions of the atom will be oscillatory, and may be left out of account in calculating the mean effect of the incident velocity. Conceive the centre (A) of the atom from which the velocity is propagated, and the centre (A′) of that on which it is incident, to be joined by a straight line, and let θ be the angle which any radius of the latter makes with this straight line. At this point of the reasoning I must take for granted what I have elsewhere much insisted upon, viz. that the reflected velocity at any point of the surface corresponding to the angle θ is $\frac{f(t)}{r^2}\cos\theta$. It may suffice to state here, that this value is required by the general law of *axes of rectilinear transmission*, which I have shown to be an inference from the general hydrodynamical equation which expresses the law of continuity of the motion. (See the article in the Philosophical Magazine for December 1852, Prop. X.) The resolved part of the velocity along the surface is consequently $\frac{f(t)}{r^2}\sin\theta$. We can therefore calculate the amount of pressure on the hemispherical surface on which the velocity is incident, by the hydrodynamical equation applicable to impressed or constrained curvilinear motion, viz.

$$\frac{\kappa^2 a^2 d\rho}{\rho ds} + \frac{dV}{dt} + \frac{V dV}{ds} = 0,$$

in which ds is the *differential of the line of motion*. But the lines of motion in this instance are the intersections of planes passing through the line AA′ with the surface of the atom. Hence, if α be the radius of the atom, $ds = \alpha d\theta$. Consequently integrating, and considering r to be constant, which is allowable on account of the small size of the atom and because the change of ρ due solely to change of r has already been considered, we obtain

$$\kappa^2 a^2 \,\mathrm{Nap.log}\,\rho - \frac{\alpha f'(t)}{r^2}\cos\theta + \frac{(f(t))^2}{2r^4}\sin^2\theta = \psi(t).$$

Determining the arbitrary quantity so that where $\theta = \frac{\pi}{2}\rho$ is equal to a constant ρ_1, because at these positions there is no re-

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flexion, we have

$$\kappa^2 a^2 \text{Nap. log}\frac{\rho}{\rho_1} - \frac{\alpha f(t)}{r^2}\cos\theta - \frac{(f(t))^2}{2r^4}\cos^2\theta = 0.$$

Let $\rho = \rho_1(1+\sigma)$. Then to small quantities of the second order

$$\kappa^2 a^2 \sigma = \frac{\alpha f'(t)}{r^2}\cos\theta + \left(\frac{\alpha^2 \cdot (f'(t))^2}{2\kappa^2 a^2 r^4} + \frac{(f(t))^2}{2r^4}\right)\cos^2\theta,$$

where the term involving α^2 may, if we please, be retained; but as it is probably wholly insignificant on account of the small magnitude of α, for the sake of brevity it will be omitted. The whole effective pressure on the atom in the direction AA′ is

$$2\pi\rho_1 \int a^2 \sigma \cos\theta \times \alpha \sin\theta \times \alpha\, d\theta,$$

taken from $\theta = 0$ to $\theta = \frac{\pi}{2}$, because by hypothesis the incident velocity is unaccompanied by condensation, so that for values of θ greater than $\frac{\pi}{2}$ the density is equal to ρ_1. Hence it will be found that the whole pressure is equal to

$$\frac{2\pi\rho_1\alpha^3 f'(t)}{3\kappa^2 r^2} + \frac{\pi\rho_1\alpha^2 (f(t))^2}{4\kappa^2 r^4}.$$

The first term of this result is periodic, on account of the factor $f'(t)$, and therefore indicates no motion of translation of the atom. The other term is partly periodic and partly non-periodic, and shows that the atom is subject to a pressure, which causes a motion of translation *from* A, and varies inversely as the *fourth* power of the distance. It is evident that this repulsion from A, is a force of a higher order than the attraction towards A, found in the former part of the investigation, because the latter was multiplied by the ratio of the radius of the atom to r. Hence the resultant of the forces acting between two atoms is a repulsion.

It may be also remarked that, since

$$f(t) = m \sin(bt + c) + m' \sin(b't + c') + \&c.,$$

$$(f(t))^2 = \frac{m^2}{2} + \frac{m'^2}{2} + \&c. + \text{periodic terms}.$$

Hence the effect of several sets of vibrations acting simultaneously, is the sum of the effects they would produce if they acted separately.

Assuming the dynamic repulsion between two atoms obtained by the preceding reasoning to be identical with the repulsion of heat, since it was found that the motion of translation was due

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to the square of the velocity of the secondary waves, and therefore to the square of the velocity of the original waves, which latter velocity varies *inversely as the distance* from a remote origin, it follows that the heating effect of a radiating hot body upon another distant body varies inversely as the *square* of the distance between them, as is found by experiment to be the case.

If the investigation had taken account of terms of the second order in the value of the velocity of the secondary waves propagated from the atom A (as terms of that order were taken into account in the value of the condensation), the effect on the motion of translation of the atom A′ would have been of the order of the *fourth* power of the incident velocity, and may therefore be neglected in comparison with the effect due to terms of the first order.

The part of the velocity accompanied by condensation, which was left out of account in the preceding investigation, has at the surface of the atom A the ratio $\frac{2\pi\beta}{\lambda}$ to the part considered, β being the radius of the atom. This ratio is so small that the effect of the omitted part may be wholly inappreciable. It is, however, to be observed that the velocity accompanied by condensation varies inversely as the distance, and consequently that its moving effect on the atom A′ would be found by a like investigation to vary inversely as the *square* of the distance. Also the condensation accompanying it would cause a propagation of velocity to the opposite side of A′, so that the integration with respect to θ would have to be taken to some value greater than $\frac{\pi}{2}$, and the effect of the velocity on the side towards A would be in part counteracted. It is not my intention to enter fully upon the consideration of the waves of condensation in this communication; but an important general remark relating to them may be appropriately made here.

If we suppose waves of condensation to be propagated from all the atoms contained in a spherical space of radius r, the condensation resulting from the composition of the waves at any distance (R) from the centre of the sphere very large compared to r will vary as the number of atoms, that is, as r^3 directly, and as R inversely, the mean interval between the atoms being given. If now we take another sphere, of larger radius, r', and another point, whose distance (R′) from the centre of the sphere is such that $\frac{r}{R} = \frac{r'}{R'}$, then the condensation at the first point is

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to the condensation at the other as $\frac{r^3}{R}$ to $\frac{r'^3}{R'}$, that is, as r^2 to r'^2, or as R^2 to R'^2. Hence, however small may be the condensation propagated from a single atom, the resulting condensation from an *aggregation* of atoms in a spherical volume may be of sensible magnitude at finite distances from the centre, if the number of atoms in a given space be very great. The dynamical effect of these compound waves on a given atom may be investigated in the same manner as the effect of the secondary waves propagated from a single atom; and a motion of translation will be found to result as before. But it is evident that according to the distribution of the condensation about the atom, resulting from propagation along its surface, the motion of translation may either be *from* the centre of the waves, or be neutralized by equality of the pressures on the two hemispheres, or be *towards* the centre by an excess of pressure on the further hemisphere. The full consideration of these cases I reserve for a future opportunity. At present it will suffice to say that in this manner it is conceivable that while the individual atoms are repulsive to each other, an aggregation of atoms may give rise to a controlling attraction.

The last inference is important, on account of its bearing on a theory of the different physical states of substances. A body in a state of *solidity* has an energetic attraction of aggregation; in *liquids* this attraction is only feebly in excess of the atomic repulsion; and in *gases*, the atomic repulsion is in excess, the density being small, and is controlled by some extraneous attraction, as that of gravity. In gaseous masses of great magnitude, such as the atmospheres of comets appear to be, the solar rays may produce a vast accumulation of heat by successive orders of internal reflexions; and it would be quite in accordance with this theory if, under these circumstances, an attraction of aggregation were generated sufficient to control the atomic repulsion. I have, in fact, shown, in a communication recently made to the Cambridge Philosophical Society, that most of the phænomena of Donati's comet, and in particular the production and direction of the tail, admit of explanation on this hypothesis.

Again, it may be remarked that the atomic repulsion from a single atom of an aëriform substance may be nearly independent of the density, on account of the comparatively large intervals between the atoms. But in the case of liquids or solids, the much greater proximity of the atoms to each other may give rise to successive orders of reflexions, which would cause the intensity of the compound waves reflected from a given atom to be dependent on the number of atoms in a given space. In fact, a difference between aëriform bodies and fluids or solids in this

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particular may very well account for what has been called *latent heat*.

It has been supposed in the theory, that the atoms from which the secondary waves are propagated, and those on which they are incident, are completely at rest. It is, however, evident that the amount of reflexion must be in some degree influenced by the mobility of the atoms, being less as the mobility is greater. Hence, as it may be supposed that when a substance is passing from a state of fluidity to a state of solidity the stability of the atoms is increasing, it is conceivable that the total atomic repulsion during this state of transition may be on the increase, although the temperature of the substance, and consequently the intensity of the waves propagated from each atom, may be decreasing. In this manner the expansion of water, with a diminishing temperature near the temperature of conversion into ice, may be explained.

Cambridge Observatory, February 18, 1859.

XXXIII. *Second Note on* Poinsot's *four new Regular Polyhedra.* *By* A. Cayley, *Esq.**

THE Note on Poinsot's four new regular Polyhedra (February Number, p. 123) was written without my being acquainted with Cauchy's first memoir, "Recherches sur les Polyèdres" (*Jour. Polyt.* vol. ix. pp. 68–86, 1813), the former part of which (pp. 68–76) relates to Poinsot's polyhedra. Cauchy considers the polyhedra, not as projected on the sphere, but *in solido*; and he shows, very elegantly, that all such polyhedra *must* be derived from the ordinary regular polyhedra by producing their sides or faces. The reciprocal method would be to produce the sides or join the vertices; and, adopting this reciprocal method, and projecting the figure on the sphere, we have the method employed by Poinsot, and explained and developed in my former Note. Cauchy does not at all consider Poinsot's generalized equation, $eS + H = A + 2E$, nor of course my further generalization, $eS + e'H = A + 2D$; but the latter part of the memoir relates to a generalization, in a different direction, of Euler's original formula, $S + H = A + 2$: viz. Cauchy's theorem is—"If a polyhedron is partitioned into any number of polyhedra by taking at pleasure, in the interior of it, any number of new vertices, and if P be the total number of polyhedra thus formed, S the total number of vertices (including those of the original polyhedron), and A the total number of edges, then $S + H = A + P + 1$; that is, the sum of the number of vertices and the number of faces

* Communicated by the Author.

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exceeds by unity the sum of the number of edges and of the number of polyhedra."

For P=1, we have Euler's equation S+H=A+2; and for P=0, we have a theorem relating to the partition of a polygon; viz. if the polygon is divided into H polygons, and if S be the number of vertices, and A the number of sides, then S+H=A+1; from which it is easy to pass to Euler's equation, S+H=A+2, for polyhedra. I remark that, in the equation S+H=A+1, H should, in analogy with Cauchy's notation for polyhedra, be replaced by P; so that we have for a single polygon,

$$A=S;$$

and for the partitions of a polygon,

$$A=S+P-1:$$

corresponding respectively to Euler's theorem for a single polyhedron, viz.

$$S+H=A+2;$$

and to Cauchy's theorem for the partitions of a polyhedron, viz.

$$S+H=A+2+(P-1).$$

Cauchy's second memoir (pp. 87–98) contains a very beautiful demonstration of the theorem implied in the ninth definition of the eleventh book of Euclid, viz. that two convex polyhedra are equal when they are bounded by the same number of faces equal each to each.

2 Stone Buildings, W.C.,
February 1, 1859.

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 147.]

June 17, 1858.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Researches on the Action of Ammonia on Glyoxal." By Dr. H. Debus.

If alcohol be slowly oxidized by nitric acid at ordinary temperatures, besides other substances, glyoxal, $C_2H_2O_2$, and glyoxylic acid, $C_2H_4O_4$*, are formed.

I have continued the investigation of these substances, and beg to lay before the Royal Society some of the more interesting results.

Glyoxal, of the consistency of ordinary syrup, is mixed with about three times its bulk of strong ammonia, and the mixture kept for twenty minutes at a temperature from 60° to 80° C. The liquid

* C=12, H=1, O=16.—Phil. Mag. Nov. 1855, and Jan. 1857.

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now contains two organic bases—one in the shape of a crystalline precipitate, which I propose to call glycosine, and the other in solution, to which in this paper the name of glyoxaline will be applied. Besides these two substances, only a little formic acid and the excess of ammonia can be recognized in the liquid.

Glycosine = $C_6H_6N_4$. The crystals contained in the ammoniacal liquid are collected on a filter and washed with cold water. By dissolving them in diluted hydrochloric acid, treating with charcoal, and adding ammonia to the decolorized solution, the glycosine is obtained as a colourless, crystalline precipitate. The crystals are little prisms, tasteless, inodorous, and only soluble in a great quantity of boiling water. They become very electric when rubbed in a mortar. A little glycosine placed between two watch-glasses and heated on a sand-bath, sublimes without leaving a residue, and produces magnificent prismatic needles, sometimes half an inch in length. It forms salts with acids, which generally crystallize well. The chloride has a great tendency to form double salts with the chlorides of copper, mercury, and platinum.

Chloroplatinate, $C_6H_6N_4 + 2(H\,Cl\,Pt\,Cl_2)$, forms a fine yellow crystalline powder, soluble with difficulty in water. An excess of water seems to abstract bichloride of platinum and hydrochloric acid. Glycosine is formed from ammonia and glyoxal according to the equation—

$$\underbrace{3(C_2H_2O_2)}_{\text{Glyoxal.}} + 4\,NH_3 = \underbrace{C_6H_6N_4}_{\text{Glycosine.}} + \underbrace{6\,H_2O}_{\text{Water.}}$$

I showed on another occasion, that glyoxal has the properties of an aldehyde. Its behaviour towards ammonia confirms my former conclusions. The formation of amarine, from oil of bitter almonds, of acetonine from acetone and ammonia, takes place in a similar manner:—

$$3(C_7H_6O) + 2NH_3 = \underbrace{C_{21}H_{18}N_2}_{\text{Amarine.}} + 3H_2O$$

$$3(C_3H_6O) + 2NH_3 = \underbrace{C_9H_{18}N_2}_{\text{Acetonine.}} + 3H_2O$$

In all other known cases, when from an aldehyde or the chloride of an alcohol radical and ammonia, a basic substance is formed, one or two equivalents of ammonia participate in the reaction.

If ammonia and glyoxal decompose each other, four equivalents of the first transfer their nitrogen to one equivalent of the base produced. The direct derivation from ammonia of a base which contains four equivalents of nitrogen, seems to me to be very interesting.

The rational formula of glycosine is probably

$$N_4 \begin{cases} C_2H_2 \\ C_2H_2 \\ C_2H_2 \end{cases}$$

C_2H_2 being equivalent to H_2.

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It is worthy of notice, that in chemical decompositions very often three equivalents of an aldehyde unite and act like one molecule. I will only mention, as examples, mesitylene, acetonia, thialdine, hydrosalicylamide, and amarine.

Glyoxaline $= C_3 H_4 N_2$ is obtained as binoxalate from the mother-liquor of glycosine, if, after expelling the ammonia by gentle heating, an excess of oxalic acid is added. The binoxalate crystallizes very well and may be purified easily. The composition of it is expressed by the formula $C_3 H_4 N_2 + C_2 H_2 O_4$. The base is obtained from this salt by treating it with carbonate of lime, and evaporating the filtrate from the oxalate of lime to the consistency of a strong syrup.

Glyoxaline crystallizes with difficulty in prismatic crystals, radiating from one centre. It is easily soluble in water, has a strong alkaline reaction, neutralizes acids perfectly, but does not appear to form a compound with carbonic acid. It melts easily, smells like fish, and evaporates at a higher temperature in dense white fumes. Chloride of copper forms a precipitate with glyoxaline, which is soluble in an excess of the base.

The chloroplatinate, $C_3 H_4 N_2 + HCl\,PtCl_2$, crystallizes in large red prisms, and is easily soluble in hot water.

The formation of glyoxaline takes place according to the following equation:—

$$\underbrace{2(C_2 H_2 O_2)}_{\text{Glyoxal.}} + 2N H_3 = \underbrace{C_3 H_4 N_2}_{\text{Glyoxaline.}} + \underbrace{C H_2 O_2}_{\text{Formic acid.}} + \underbrace{2H_2 O}_{\text{Water.}}$$

Glyoxaline is homologous with sinnamine.

"Further Remarks on the Organo-metallic Radicals, and Observations more particularly directed to the isolation of Mercuric, Plumbic, and Stannic Ethyle." By George Bowdler Buckton, Esq., F.R.S.

Before again entering on the subject of the organo-metals, the author wishes to call attention to the remarks he has previously made* on the difficulties which presented themselves at that time in the preparation of mercuric ethyle. Secondary decompositions, induced by the nature of the materials employed and the high temperature necessary to the reaction, showed themselves even in the more easily prepared mercuric methyle, and reduced the quantity obtained considerably below that pointed out by theory.

The loss sustained in the similar operation of distilling together cyanide of potassium and iodide of mercurous ethyle, $C_4 H_5 Hg_2 I$, is yet more marked; and it may be remembered that the portion obtained did no more than suffice for a cursory examination of its most marked characters. A new mode of operating was therefore desirable, and it was not long before the following considerations presented themselves.

The powerful and well-defined affinities of zinc-ethyle have already furnished a valuable key to the explanation of several chemical

* Phil. Trans. Roy. Soc.; Proc. Roy. Soc. vol. ix. p. 91.

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problems, and seem to be well suited for experiment in the present case. Bearing in mind its well-known reactions on water and hydrochloric acid, there appeared to be well-grounded reasons for supposing that interesting decompositions might be effected with various oxides, chlorides, and iodides.

Through the instrumentality of zinc-ethyle the author has succeeded in isolating, in a neat and efficient manner, several of the organo-metals, and he indulges a hope that they may, when taken as starting-points of investigation, prove of service in fixing exact formulæ to some of those bodies, the composition of which, at present, appear doubtful from their complexity.

Action of Zinc-ethyle on Mercuric Chloride.

Corrosive sublimate acts with great energy on zinc-ethyle; so much so, as to render it necessary to cool the apparatus in water, and add the well-dried salt by degrees. An excess of the latter must be avoided, since chloride of mercurous ethyle would be formed, as was formerly shown to be the case in the methyle series.

After the two bodies have been brought together in their proper proportions, heat is applied, and the radical passes over by distillation as a heavy, colourless, and nearly inodorous liquid; the slight excess of zinc-ethyle is then decomposed by the addition of water, and just sufficient dilute hydrochloric acid added as will dissolve the precipitated oxide of zinc.

The two transformations may be seen in the equations,

$$C_4 H_5 Zn + Hg Cl = C_4 H_5 Hg + Zn Cl,$$

and again,

$$C_4 H_5 Hg + Hg Cl = C_4 H_5 Hg_2 Cl.$$

The pure radical boils at a temperature between 158° and 160° C. It burns readily, with a luminous and somewhat smoky flame, with disengagement of mercurial vapour. It is almost wholly insoluble in water. Alcohol dissolves it rather sparingly, but it mixes freely with ether.

The behaviour of acids towards mercuric ethyle is strictly analogous to that shown by mercuric methyle. With dilute acid there is but little change, but warm concentrated hydrochloric or sulphuric acid liberates hydride of ethyle in sufficient quantity to permit of its inflammation through a gas jet. The salts of mercurous ethyle remain in solution.

The specific gravity of a specimen boiling between 158° and 160° C. was found to be 2·444, and the same sample when submitted to analysis, gave numbers agreeing accurately with the formula

$$C_4 H_5 Hg.$$

The correctness of this formula was further confirmed by an appeal to the vapour-density.

The first experiment failed, from the circumstance that the vapour decomposes with a slight explosion, when heated a few degrees above 205° C. In this experiment metallic mercury was deposited

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on the walls of the glass balloon as a grey film, and the other contents consisted of an inflammable gas. Mercuric ethyle appears therefore to be resolved at this temperature into ethyle gas and mercury.

Another experiment was more successful, and gave the number 9·97 for the vapour-density.

The equivalent weight of mercuric ethyle is 129, which, being divided by the former figures, gives $\frac{129}{9\cdot97} = 12\cdot94$. If the constituents of this radical be condensed into two volumes of vapour, the more accurate number 14·86 should have been obtained.

The theoretical density of mercuric ethyle, thus calculated, is equal to $\frac{129}{14\cdot86} = 8\cdot68$*.

This portion of the subject would be incomplete unless a few words were added on the behaviour of zinc-ethyle towards mercurous chloride.

It has been mentioned, that all attempts to reduce iodide of mercurous methyle to the form of a radical containing one equivalent of methyle and two equivalents of mercury have hitherto failed.

Reasoning *a priori*, we should not expect to find a departure in the present case, neither does such appear. Mercurous chloride reacts with vigour on zinc-ethyle, but metallic mercury is formed simultaneously with chloride of zinc and mercuric ethyle.

The decompositions of mercurous and mercuric chlorides or iodides, are thus shown:—

$$C_4 H_5 Zn + Hg_2 Cl = C_4 H_5 Hg + Zn Cl + Hg,$$

and

$$C_4 H_5 Zn + Hg Cl = C_4 H_5 Hg + Zn Cl.$$

Having succeeded, by these simple means, in effecting a replacement in zinc-ethyle through the ordinary metallic chlorides, there remained yet one point untouched, viz. the behaviour of various organo-metallic salts, under similar treatment.

First in order was tried

The Action of Zinc-ethyle on Iodide of Mercurous Ethyle.

Carbonic acid, or ordinary coal-gas, was slowly passed through the neck of a retort; and when the atmospheric air was displaced, about two ounces of zinc-ethyle, nearly free from ether, and wholly so from iodide of ethyle, was introduced. Iodide of mercurous ethyle was then added, by degrees, through the tubulure, and the whole mixed by agitation. The zinc-ethyle at first dissolves the iodide, but subsequently a cake of iodide of zinc is formed. Distillation was then commenced, the

* Here it is fitting to mention an error that has crept into the calculation of the vapour-density of mercuric methyle as it appears printed in the 'Proceedings of the Royal Society.' A false figure in the denominator of one of the fractions, causes the experimental density to appear as 14·86, whereas the true experimental density observed was 8·29. The theoretical density of mercuric methyle, calculated for two volumes, equals $\frac{115}{14\cdot46} = 7\cdot95$.

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heat being raised by degrees until gaseous products appeared. The distillate, after being well washed, was rectified by the thermometer, and in this manner the radical was obtained in a state of purity. Iodide of mercurous ethyle may be formed so easily by diffused daylight, and its action is so gentle on zinc-ethyle, that its use offers greater conveniences to the operator than are afforded by any of the substances previously mentioned.

For obvious reasons, a similar choice of materials is recommended for preparing mercuric methyle.

Action of Zinc-ethyle on Chloride of Lead.

The close relations which exist between the three metals, lead, mercury, and silver, in their equivalent weights, salts, and other characters, lead the author to anticipate success in forming their ethyle bases.

The existence of the lead radical might indeed be considered as certain, since various salts of complicated structure have been made known to chemists through the experiments of M. Löwig, on the alloy of lead and sodium, under treatment with iodide of ethyle.

The principal product obtained by him, and the only one apparently analysed, had a grouping similar to a sesquichloride. The formula ascribed by him to the radical plumbethylium is $Pb_2 (C_4H_5)_3$. I have attempted to form the iodide of this radical by exposing sealed tubes, containing granulated lead and iodide of ethyle, to the sun's rays, but without success. No better result was obtained by substituting bromide of ethyle for the iodide, and no change could be induced even when these tubes were heated strongly with high-pressure steam.

M. Löwig's method was not resorted to, from the supposition that the action of zinc-ethyle on a mixture would only give rise to radicals of various constitution, which it might be impossible afterwards to separate, except by working on a large scale, which, considering the costliness of the materials, had its disadvantages. Perhaps success might attend the use of one of Dr. Frankland's mirrors for concentrating the sun's rays.

For obtaining the lead-radical, recourse was had to well-dried chloride of lead, which was introduced into a flask containing zinc-ethyle. The chloride immediately turned black, from the deposit of metallic lead, whilst moderate heat was disengaged. An excess of chloride was used, and the mass incorporated by stirring with a glass rod. After applying a gentle heat for a few minutes, the floating clear liquid was pipetted off. This substance is apparently a compound of zinc-ethyle and the lead radicals. It fumes slightly in the air, and no digestion with chloride of lead appeared to resolve it entirely into the lead base.

A great part of the zinc-ethyle, however, is removed by subsequent distillation; but the temperature should not be permitted to rise above 140° or 150° C. The substance in the retort is then treated with water and dilute hydrochloric acid, when the radical separates, and sinks in the form of colourless drops. When distilled cautiously,

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the thermometer soon rises to 200°; but beyond this point the vapour is very prone to decomposition, with deposit of metallic lead.

From this tendency to change, there is some difficulty in obtaining the substance wholly pure from bodies with lower boiling-points. The larger portion came over between 198° to 202°. Its specific gravity was found to be 1·55.

Analysis led to the formula

$$Pb\,C_8\,H_{10}, \text{ or } Pb\,(C_4\,H_5)_2.$$

It should, however, be noticed that a trifling excess in the percentage of carbon obtained, showed an increase rather than a decrease in the number of equivalents of ethyle.

This radical, for which the provisional name of plumbic bis-ethyle is suggested, is a colourless fluid, possessing little or no odour. It is insoluble in water, but perfectly miscible with ether. It burns readily with a beautiful orange-coloured flame, edged with blue, and gives off fumes of oxide of lead.

The radical appears to be incapable of forming salts without a partial decomposition. With weak acids there is no perceptible action; but when they are concentrated and gently heated, a gas is given off, and crystalline salts are produced.

The chloride is insoluble in water, but soluble in alcohol and in ether, from which last liquid it crystallizes in satiny needles, which are very volatile and provoke sneezing and lachrymation. It burns with the characteristic lead flame, and by long digestion with concentrated hydrochloric acid, is converted into chloride of lead and volatile products.

The sulphate also appears as a crystalline mass when plumbic-bis-ethyle is gently warmed with a few drops of concentrated sulphuric acid. It is conveniently prepared by agitating the materials in a stoppered bottle, an exit being made from time to time for the gas which is liberated.

Both these salts require analyses to fix their composition, the details of which the author hopes shortly to be able to communicate.

The Action of Zinc-ethyle on Chloride of Silver.

These substances react with some violence, and a black substance sinks in the liquid, which proved to be a mixture of chloride and metallic silver. The zinc-ethyle seems partly to escape decomposition, even when the chloride is in excess and considerable heat is applied. On the addition of water, effervescence sets in, and chloride of zinc is alone found in solution.

In another experiment dry ether was employed instead of water, under a supposition that a solid compound might be formed, soluble in that menstruum. The only reaction, however, appeared to be that expressed by the equation,

$$C_4\,H_5\,Zn + Ag\,Cl = Zn\,Cl + Ag + C_4\,H_5.$$

A similar negative result was obtained when zinc-ethyle was made to react on protochloride of platinum, Pt Cl. The action is violent, and the platinum is thrown down in the form of platinum-black.

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The same remark also applies to protochloride of copper, Cu_2Cl, when similarly treated; no combination of copper and ethyle could be thereby eliminated.

Action of Zinc-ethyle on Iodide of Stan-ethyle.

This iodide, C_4H_5Sn,I, was readily obtained by heating sealed tubes containing excess of tinfoil and iodide of ethyle from 150° to 160° C. The pure transparent crystals which were obtained by a little management, were introduced, in a melted state, into a retort containing zinc-ethyle. It is necessary to cool the apparatus with water. After breaking up the resulting mass, the retort was heated until the thermometer marked 210° C., and the distillate, which contained a slight excess of zinc-ethyle, was agitated with water, and treated with dilute acid, as before described.

The resulting heavy liquid was again distilled, and fractionized with the thermometer. By far the larger portion came over between 170° and 180° as a clear and colourless body, insoluble in water, but soluble, like the other radicals, in ether. That portion which possessed a boiling-point between 176° and 180° C., was taken for examination, and was found, when burned with oxide of copper, to give the formula

$$Sn\,C_8\,H_{10},\text{ or }Sn\,(C_4H_5)_2.$$

This compound, for which the name stannic bis-ethyle is proposed, has a specific gravity of 1·192. In its external and more prominent characters it resembles plumbic bis-ethyle; but an exception may be made, that it is more stable. It is very combustible, burning with a coloured flame and scintillation like that exhibited by the metal tin under the flame of the hydro-oxygen blowpipe.

This radical appears to differ in several particulars from the organo-metal stan-ethyle, C_4H_5Sn, obtained by Dr. Frankland by acting on sheet-zinc with a salt of stan-ethyle. This last body is described as a thick, oily substance, possessed of a powerful odour, and having a specific gravity of 1·55. It differs also in its lower boiling-point, which is about 150° C.

Pure stannic bis-ethyle is perfectly limpid, inodorous, and is acted upon by hydrochloric acid with difficulty. A gas is slowly evolved on the application of heat, and a chloride is formed which seems to be richer in tin than the radical itself.

The chloride appears to crystallize with difficulty, and at usual temperatures has the consistence of an oil. It possesses a powerfully pungent odour, and when heated, a vapour which painfully attacks the skin of the face, and produces fits of sneezing.

A corresponding bromide is formed when bromine is added to stannic bis-ethyle. It is an oily body, with an irritating odour. When acted upon by ammonia, an oxide is precipitated, which with acids forms beautiful crystallizable salts, readily soluble in water.

A complete history of these salts, and their decompositions with zinc-ethyle, will possess much interest, and may prove of value in referring to a few simple radicals the numerous complex bodies described by Löwig, &c.

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The author is at present engaged on this branch of the inquiry, a detailed account of which he hopes to embody in a communication to the Royal Society, the present paper being intended only as an outline to be hereafter filled in.

In conclusion, the author would remark that a rich harvest can scarcely fail to be reaped, from submitting to the action of zinc-ethyle the metallic compounds of other groups, such as arsenic, bismuth, and antimony.

"Preliminary Notice of Additional Researches on the Cinchona Alkaloids."—Part III. By W. Bird Herapath, M.D.

Having had occasion to make some experiments upon the rotatory power of the β-quinidine mentioned in the first part of his paper*, he arrived at the conclusion that some other feebly dextro-gyrate alkaloid accompanied it, and of a more soluble and less crystallizable character. Consequently, on its further purification by frequent recrystallization from alcohol, the quinidine was obtained perfectly pure; it then had the molecular rotation assigned to it by Pasteur, namely 250°·75↗. Two examinations have given the following elements:—

I. Its solution having been made in rectified spirit of ·836 by boiling, and crystallized at 62° F., the concentrated solution decanted gave the following elements for Biot's formula:—

e.	δ.	l.	Arc blue violet.	
·02728	·85172	315·8	18°·5↗ =	251°·7↗

II. Its sulphate, perfectly neutral, and concentrated at 61° F.:—

e.	δ.	l.	Arc.	
·0088441	1·00406	315·8	7°↗	=249.55↗

These observations were all made with the naked eye, and the tint of passage was that of the blue-violet petal. When the pink violet, or lilac tint was employed, the arc observed was 20°·25 for No. I. experiment, which with the same elements of calculation gave 274°·093↗; and with No. II., the arc 25°·75, which, as before, gave 279°·7↗. The slightly dextro-gyrate alkaloid existing as a contamination was quinicine; and upon its removal, the β-quinidine had the same solubility in ether as the quinidine of Pasteur. One very peculiar circumstance elicited during this examination, was the fact that the perfectly pure recrystallized quinidine, if made into the neutral sulphate and crystallized by cooling, produces, if made with distilled water at 212° F., a slightly greenish solution, however great the care which might have been taken to remove all the mother-water by washing the crystal on the filter. This green tint deepens considerably during concentration, or by boiling, and at length gives rise to the erroneous impression that some salt of copper is present: in this condition, a tube having a length of 315·8 millims., when

* Phil. Mag. vol. xvi. p. 55.

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filled with the solution, is absolutely impervious to light. It is probable that some molecular change is produced by the action of boiling, even if only for a short time; therefore it was necessary to make a concentrated solution at 120° F., and set in repose for some days at 62° F., by which precaution the solution experienced only a very slight discoloration. When formerly experimenting on β-quinidine, the author obtained an iodo-sulphate very different from that which he has described as indicative of the quinidine of Pasteur: having pursued this inquiry, he is now enabled to state that his former discrepancies arose from the fact that quinidine forms two iodo-sulphates, according to the manner in which it is treated.

1st. When a dilute solution of the acid sulphate of quinidine is mixed with one-third or one-half its bulk of rectified spirit and raised to 160° or 180°, then treated with tincture of iodine in small quantities, the red iodo-sulphate is produced, having the characters previously described as indicative of quinidine,—quinine, when similarly treated, invariably producing the optical salt.

The only precaution necessary to be taken in the case of the alkaloid quinidine is to avoid adding an excess of iodine; otherwise an amorphous resinoid substance is deposited which will not crystallize.

2ndly. But when we treat the acid sulphate of quinidine in a concentrated form, diluted with from thirty to forty times its bulk of rectified spirit at a temperature rather below 120° F., with the tincture of iodine, even in greater proportions, an optical salt of quinidine is produced, being the perfect analogue of the quinine salt.

It crystallizes from this strong spirituous solution in acicular long lanceolate prisms, the form of which appears to be a rhomboid having 30° for the acute and 150° as the obtuse angles; but they are more frequently found with a termination like the blade of an ordinary bleeding-lancet. These prisms have a frequent disposition to hemitropism, but in superposition, so that two plates may be often found *overlying each other in a parallel position*, wholly obstructing light in those portions *where they cover each other*, but transmitting an olive- or yellowish-green tint where separate.

Sometimes the terminal planes are rectangular. This imbricated mode of crystallization is very peculiar; and although the author has made thousands of experiments with quinine, yet he never saw anything similar to it; for this alkaloid invariably crystallizes from *dilute alcoholic* solutions as the α-prism, obstructing light when the length is perpendicular to the plane of reflected light polarized in a vertical plane,—or from *strong* alcoholic solutions, where the spirit is about two-thirds the bulk, as β-prisms, which obstruct light in the opposite plane, or, as the author has described it, when the planes of their length "lie in a plane parallel to that of the polarized beam with which they are examined." In the case of quinine, these two sets of prisms never occur together; but if made by separate operations *and then artificially mixed on the same slide*, they present the optical characters of this new quinidine salt, viz. *obstructing light when two long prisms overlie each other in a parallel position*. They are there-

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fore α- and β-prisms crystallizing together from the *same strong* alcoholic solution.

The more frequent form in which this salt shows itself, however, is as the α-prism, from solutions in which the alcohol is vastly predominant over the water; whereas with quinine, β-prisms always develope themselves under similar circumstances.

This new quinidine salt has a very great similarity in its optical property to the quinine salt. Its reflected tint is a metallic blue-green, when in liquid or in contact with glass; but after filtering, and when exposed on paper, it has a brownish-olive colour, and loses all appearance of metallic reflexion to the naked eye. Its transmitted tint is, when polarized parallel to its axis, a brownish-yellow green, even in thin plates, but verging to brown in thicker. Its "indicative body-colour" is brownish red.

One great peculiarity attends upon this salt; if it be permitted to remain in the acid mother-liquid, it disintegrates by gradual solution, and disappears, whilst, upon the side of the bottle, solid and large crystals slowly form, of a rhombohedric form, or having some of its modifications, the more frequent of which is that with replacement upon the short axis of the rhombohedron by triangular planes. These crystals have a deep sienna-brown colour by transmission, and a dark steel-blue by reflexion, verging on purple; they strongly polarize light, and differ materially from the garnet-red iodo-sulphate previously described, by the greater intensity of their optical properties.

When we attempt to purify the optical thin prisms by recrystallization from alcohol, the same modification appears to be produced; but the crystals are acicular rhombic prisms; the optical characters are the same, however, as those of the rhombohedral form.

The characters, therefore, by which this salt is known from quinine are many.

1st. Its crystallizing as α-prisms, or as α- and β-prisms from strong spirituous solutions.

2nd. Its brownish-olive reflected tint as seen by the naked eye.

3rd. Its deeper yellow and brownish-green transmitted tint.

4th. The probable difference in the primary form of the laminated variety, being a very acute prism of a rhombic form, having 30° as the acute, and 150° as the obtuse angles.

5th. The modification which it undergoes by resolution or recrystallization, and the formation of a salt more resembling the garnet-red iodo-sulphate, but having strongly marked differential characters from this beautiful salt, viz. its strong tourmaline powers of absorption and its deeper colour, being nearly a brown-purple, and by its disposition to assume the rhombohedric form.

The author has not yet analysed this salt, but hopes ere long to accomplish this matter and communicate his results to the Royal Society; but he ventures to hope that it will be found to contain 2 atoms sulphuric acid and 3 atoms iodine, like the analogous quinine and cinchonidine salts.

The author has also assured himself that there is an analogous

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class of salts produced by ethyle-quinine and ethyle-quinidine, but optically distinct from those of quinine and quinidine. He has already produced three salts from ethyle-quinine, having optical characters different from any previously described.

1st. A deep purple-red salt by transmitted light, in thicker plates or aciculæ quite impervious to light. This salt occurs as very slender acicular prisms; it has a brilliant metallic-green reflected tint, but very little double absorption.

2nd. There is a foliaceous salt, having a plate-like form, a deep red or orange-red colour, transmitting an orange-yellow, having only slight optical powers.

3rd. A salt having many of the characters of the new quinidine salt when first produced, viz. the optical characters and the α-form; but on attempting to recrystallize it, the orange-red plates just described are alone produced.

The only salt yet produced from ethyle-quinidine is one very similar to the red salt described above, but it has only been very partially examined. The iodide ethyle-quinidine is a very beautiful silky salt, less soluble than the iodide ethyle-quinine. The author is not aware that it has yet been described. It is readily made by mixing an alcoholic solution of quinidine with an etherial solution of iodide-ethyle; on repose, the new iodide ethyle-quinidine separates in long, slender, silky aciculæ; and further crops can be repeatedly produced by diluting the original solution with water until precipitation *begins* to follow; on long repose, the iodidē crystallizes and may be removed by filtration, and washed with dilute spirit.

Note.—In reference to the rotatory power of the cinchona alkaloids, the calculation of the molecular rotation gives an excellent plan of deciding on *the purity* of the alkaloid employed; for if the absolute molecular rotation be obtained precisely identical with those given by other optical chemists, the purity may be inferred as proved. But it is possible for a large quantity of two alkaloids to be present in solution, one dextro-, the other levo-gyrate, and in such proportions that the polariscope shall give no indication of the presence of either.

Thus a highly concentrated solution of the acid sulphate of quinine, marking a left-handed rotation of 57°, was mixed with rather more than double its bulk of a similar solution of quinidine marking 24°. The resultant solution gave no rotation at all, the one effect perfectly neutralizing the other.

In experimenting upon *non-fluorescent* solutions of quinine or quinidine in the polariscope, it was found that these solutions were still possessed of their original molecular rotation upon plane-polarized light, even undiminished, if care were taken not to dilute the fluid when destroying the fluorescence by the soluble chloride, &c., which was always done by adding it in the solid state.

"Sur la Relation entre les Courants induits et le Pouvoir Moteur de l'Electricité." By Professor Carlo Matteucci of Pisa.

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"On the Influence of Temperature on the Refraction of Light." By Dr. J. H. Gladstone, F.R.S., and the Rev. T. P. Dale, M.A., F.R.A.S.

Those who have occupied themselves with the determination of refractive indices, must have noticed that changes of temperature influence the amount of refraction; yet few of the observations on record have affixed to them the temperature at which they were made, and few, if any, numerical researches have been published on the subject. To determine, if possible, the amount and character of this effect of heat was the object of the present inquiry.

The instrument employed was that described by the Rev. Baden Powell in the British Association Report for 1839, and was kindly lent by him for the purpose. The substances more or less fully examined, were bisulphide of carbon, water, ether, methylic, vinic, amylic, and caprylic alcohols, the two principal constituents of creasote—hydrate of phenyle and hydrate of cresyle, phosphorus, oil of cassia, and camphor dissolved in alcohol.

Of the tabulated results the following two will suffice to illustrate the main conclusions:—

Bisulphide of Carbon.

Temperature.	Refractive Index of A.	Refractive Index of D.	Refractive Index of H.	Difference per 5° C. for D.	Length of spectrum.	Dispersive power.
0° C.	1·6217	1·6442	1·7175		·0958	·01487
				·0045		
5	1·6180	1·6397	1·7119	·0051	·0939	·01468
10	1·6144	1·6346	1·7081	·0043	·0937	·01477
15	1·6114	1·6303	1·7035	·0042	·0921	·01462
20	1·6076	1·6261	1·6993	·0041	·0917	·01463
25	1·6036	1·6220	1·6942	·0040	·0906	·01460
30	1·5995	1·6180	1·6896	·0040	·0901	·01457
35	1·5956	1·6140	1·6850	·0037	·0894	·01456
40	1·5919	1·6103	1·6810	·0050	·0891	·01460
42	1·5900	1·6083	1·6778		·0878	·01443

Water.

0° C.	1·3293	1·3330	1·3438		·0143	·00429
				·0001		
5	1·3291	1·3329	1·3436	·0002	·0145	
10	1·3288	1·3327	1·3434	·0003	·0146	·00439
15	1·3284	1·3324	1·3431	·0004	·0147	
20	1·3279	1·3320	1·3427	·0003	·0148	·00446
25	1·3275	1·3317	1·3420	·0006	·0145	
30	1·3270	1·3309	1·3415	·0006	·0145	·00438
35	1·3264	1·3303	1·3410	·0009	·0146	
40	1·3257	1·3297	1·3405	·0008	·0148	·00449
45	1·3250	1·3288	1·3396	·0009	·0146	
50	1·3241	1·3280	1·3388	·0012	·0147	·00448
55	1·3235	1·3271	1·3380	·0010	·0145	
60	1·3223?	1·3259	1·3367		·0144	·00442
65	1·3218	1·3249		·0012	·0138	
70	1·3203	1·3237	1·3344	·0012 (A)	·0141	·00435
80	1·3178	.....	1·3321		·0143	.....

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The following are the conclusions arrived at:—

1. In every substance the refractive index diminishes as the temperature increases. This is seen in the first four columns of the tabulated results, which represent the refractive indices of the fixed lines of the spectrum A, D, and H respectively at the temperatures indicated, while the succeeding column shows the amount of difference for each five degrees Centigrade. This change of refractive index by heat, for which the term *sensitiveness* is proposed, varies greatly in amount in different substances, melted phosphorus and bisulphide of carbon being the most, and water the least sensitive of the liquids examined.

2. The length of the spectrum varies as the temperature increases. The difference between the refractive indices of the lines A and H, or $\mu_H - \mu_A$, is taken as the measurable length of the spectrum, and is given in the sixth column. In the case of highly dispersive substances, as bisulphide of carbon and hydrate of phenyle, it decreases considerably; in the case of less dispersive bodies, as the alcohols, it decreases to a less extent; while with water the change is not appreciable.

3. In some substances the dispersive power is diminished, in others it is augmented by a rise of temperature; that is, in such substances as bisulphide of carbon, it is the numerator of the fraction $\frac{\mu_H - \mu_A}{\mu_D - 1}$ that decreases fastest, while in such substances as water it is the denominator. The result of this is shown in the last column.

4. The sensitiveness of a substance is independent of its specific refractive or dispersive power. Thus water and ether are very similar as to the actual amount of the refraction and dispersion exhibited by them, but ether is many times more sensitive to heat than water is.

5. The amount of sensitiveness is not directly proportional to the change of density produced by alterations of temperature; yet there is some relationship between the two phenomena. Thus in water the index of refraction and the density both change much more rapidly at high than at low temperatures; again, the remarkable reversion of the increase of density that takes place at 4° C. is not without its indication in the amount of sensitiveness; and the large decrease of density at the freezing of water is accompanied by a similar decrease of refraction.

Substance.	Mean refraction ($\mu_D - 1$).	Specific gravity.	Ratio.
Ice	0·3089	0·9184	2973
Water at 0° C.	0·3330	0·9993	3001

Moreover, as a general rule, those substances that are most affected in density by heat are the most sensitive.

6. No sudden change of sensitiveness occurs near the boiling-point; at least this is true in respect to bisulphide of carbon, ether, and methylic alcohol.

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"On the Adaptation of the Human Eye to varying Distances." By Charles Archer, Esq., Surgeon, Bengal Army.

The following is a summary of the author's views on the question :—

1. The eye is adapted to varying distances principally by an alteration in the fibrous arrangement of the lens itself. Moreover, that when the lens is removed after an operation for cataract, the power of adaptation is nearly lost, and can only be exerted within very confined distances.

2. That the purpose of focalizing light at short distances is doubtless assisted, as suggested by Bowman, by the contractions of the ciliary muscle, in its antero-posterior direction, bringing forward the ciliary processes.

3. That as the posterior hemisphere of the capsule is firmly united to the hyaloid membrane, this portion must always remain quiescent, and therefore the antero-posterior contractions of the ciliary muscle must be very limited as regards the lens.

4. That the ciliary muscle, being placed around the eye, and its fibres being of a somewhat plexiform character, the contractions of the muscle will relax those yielding portions of the eye placed within its circumference.

5. That the relaxations of the ciliary processes will deprive the capsule of its firm support. It will be pressed forward by the lens, which will meet with no further resistance to the expansion of its short axis.

6. That the lens itself, as microscopically described by Bowman and Kölliker, is admirably adapted to the varying changes which take place in the capsule.

7. That the posterior capsule being firmly united to the hyaloid membrane, the alteration in the diameters of the cavity of the capsule must take place from the periphery of the lens to its centre, and from behind forwards, but not from before backwards, on account of the close union of the posterior capsule to the hyaloid membrane.

8. That to allow such alteration to take place without endangering the achromatism of the lens, the alterations in the plane of its long diameter must be synchronous with the alterations in the plane of its short diameter. To allow of this, the margin of the lens is free in the canal of Petit; were it not the case, chromatic aberration would result.

9. That the elasticity of the capsule of the lens and the ciliary muscle are antagonistic; that on the ciliary muscle becoming relaxed, the capsule of the lens is free to exert that elasticity.

10. That, by the pressure exerted by the anterior hemisphere of the capsule by means of the polygonal cells of Virchow on the anterior face of the lens, the organ is able to fulfil all the requirements for adapting it to receive focalized light from long distances.

11. That the polygonal cells of Virchow are placed on the posterior surface of the anterior hemisphere of the capsule with the view before mentioned, and that they are arranged with their long diameters in an antero-posterior direction, that pressure may not injure their transparency, which would be the case if placed laterally.

12. That these cells are not found in other parts of the capsule.

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13. That the fibres of the lens are serrated for the purpose of uniting either to other, so as to allow them greater freedom of motion without altering their ultimate relations to each other.

14. That the ciliary muscle is very highly endowed with nervous matter to supply all these varying requirements.

15. By the above postulates, all the modern discoveries in the microscopical anatomy of the eye receive a distinct expression of their individual functions, and, by so doing, adapt the organ of vision to the acknowledged laws of light.

"Researches on the Foraminifera."—Part. III. On the Genera *Peneroplis*, *Operculina*, and *Amphistegina*. By W. B. Carpenter, M.D., F.R.S. &c.

"Further Researches on the Grey Substance of the Spinal Cord." By J. Lockhart Clarke, Esq., F.R.S.

"On some new Ethyle-compounds containing the Alkali-metals." By J. A. Wanklyn, Esq.

The very remarkable composition and properties of that class of substances comprehending kakodyle and zinc-ethyle, have justly attached no ordinary degree of interest to the so-called organo-metallic compounds.

Influenced by that interest, I was led to inquire whether the series might not include members into whose composition the alkali-metals entered. It was a question whether combination between so powerfully electro-positive a body as potassium or sodium on the one hand, and a hydrocarbon radical on the other, did not involve impossible conditions. It seemed that the answer to this query would not be valueless as a contribution to the store of facts out of which we may hope some day to evoke the conditions of chemical combination.

My researches in this direction have already enabled me to produce combinations of ethyle with potassium and sodium; and I have little doubt that I shall be able to produce similar compounds containing lithium, barium, strontium, calcium, and magnesium. Combinations containing methyle in place of ethyle will also be sought. The present paper will be devoted chiefly to the ethyle-compound of sodium.

Sodium-ethyle.

Experiments made with a view to the formation of this body by reactions similar to that by which zinc-ethyle is produced, yielded negative results; but some months ago I made the observation that potassium and sodium decomposed zinc-ethyle, and I found the action to consist in the replacement of a portion of the zinc by the metal employed.

Sodium-ethyle was prepared as follows:—A tube of soft glass was closed at one end and filled with coal-gas. In it was then placed a single clean piece of sodium; its open extremity was then closed with the finger, and whilst still filled with coal-gas, the tube was contracted about the middle, drawn out and bent twice at right angles; pure zinc-ethyle, in quantity about ten times the weight of

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the sodium, was next introduced, and the tube hermetically sealed. So prepared, the apparatus was afterwards placed in cold water, and left therein for several days, being cautiously shaken up at intervals.

During this time the following changes were noted in the contents of the tube. The sodium became coated with zinc, and gradually disappeared, whilst the total volume of the solid and liquid contents diminished considerably. The liquid became also viscid, and sometimes separated into two portions non-miscible with each other, becoming, however, homogeneous as the operation advanced. There was no evolution of gas.

After the lapse of some days the apparatus was found to contain metallic zinc and a clear colourless liquid. The former was weighed and found to correspond to the sodium dissolved, one equivalent of zinc being precipitated for each equivalent of sodium dissolved.

The clear liquid was made the subject of special examination. It consisted of zinc-ethyle holding in solution a crystalline compound containing sodium, zinc, and ethyle. It was inflammable to the last degree, burning explosively, on exposure to the air, with a yellow flame, and leaving a very alkaline residue. Owing to its extreme tendency to become oxidized, its manipulation presented great difficulties. It was requisite to decant it into bulbs filled with dry hydrogen or coal-gas; and since heat produced partial decomposition, the bulbs had to be double, so that the heated bulb might not receive the liquid.

The clear liquid deposited large quantities of beautiful crystals when cooled to zero; and when gently warmed in a stream of dry hydrogen gas, so long as zinc-ethyle came off it yielded also a mass of crystals. Some crystals were prepared in the latter manner; they fused at about 27° C., but once fused they remained fluid at several degrees below that point. Numerous analytical determinations prove that these crystals contain two equivalents of zinc for every equivalent of sodium, and that their formula is

$$\left.\begin{matrix}\mathrm{Na}\\ \mathrm{C_4H_5}\end{matrix}\right\}, 2\left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{C_4H_5}\end{matrix}\right.$$

The reaction by which they are produced may be thus expressed:

$$6\left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{C_4H_5}\end{matrix}\right\} + \left\{\begin{matrix}\mathrm{Na}\\ \mathrm{Na}\end{matrix}\right\} = \left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{Zn}\end{matrix}\right\} + 2\left(\begin{matrix}\mathrm{Na}\\ \mathrm{C_4H_5}\end{matrix}\right\}, 2\left\{\begin{matrix}\mathrm{Zn}\\ \mathrm{C_4H_5}\end{matrix}\right).$$

For the body $\mathrm{Na\,C_4\,H_5\,I}$ I propose the name *sodium-ethyle*, and for the crystals that of *double compound of sodium-ethyle with zinc-ethyle*.

Many attempts were made to obtain sodium-ethyle free from zinc-ethyle, but without success.

By distillation it was found to be equally impossible either to distil off $\left.\begin{matrix}\mathrm{Na}\\ \mathrm{C_4H_5}\end{matrix}\right\}$ from the crystals, or to distil off all $\left.\begin{matrix}\mathrm{Zn}\\ \mathrm{C_4H_5}\end{matrix}\right\}$ so as to leave pure $\left.\begin{matrix}\mathrm{Na}\\ \mathrm{C_4H_5}\end{matrix}\right\}$ behind. When the crystals are moderately heated in a bulb, a singular phenomenon occurs. Gas is evolved, and there remains behind metallic sodium, also metallic zinc, but no car-

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bonaceous residue. This reduction of a sodium-compound by heat alone is an anomaly in chemistry.

When the crystals are heated in the water-bath with potassium, a sudden evolution of gas occurs, and there results metallic zinc, with a liquid alloy of potassium and sodium—a result likewise peculiar.

When the crystals are heated in the water-bath with excess of sodium, evolution of gas likewise takes place.

From these experiments it would seem that the conjoined zinc-ethyle is necessary to the existence of sodium-ethyle; or more precisely, that some adjunct of a less positive nature than sodium-ethyle is requisite to make the existence of the latter possible.

Passing on to the other reactions of the crystals $\left.\begin{matrix}2(ZnC_4H_5)\\NaC_4H_5\end{matrix}\right\}$:—

With water there is given pure hydride of ethyle, and hydrated oxides of zinc and sodium. The reaction takes place with great evolution of heat.

With carbonic acid there is given propionate of soda, which unites with zinc-ethyle forming a double compound, decomposed on the addition of water. To the account of this reaction, published elsewhere, I have to add that it takes place without evolution of ethyle or any other gas—a result which further confirms the formula of sodium-ethyle adopted in this paper.

With carbonic oxide there is also a reaction, which is in course of examination.

Cyanogen gas is instantly absorbed, with the formation of a brown solution.

With ether there seems to be no reaction. For the rest, with oxygen, iodine, &c., I should predict reactions quite analogous to those of zinc-ethyle, but have not specially examined the point.

Potassium-ethyle.

Zinc-ethyle and potassium react still more readily than the former body and sodium. So far as at present ascertained, the cases greatly resemble one another. Just as with sodium, I obtain crystals readily soluble in zinc-ethyle, which contain in this case abundance of potassium.

Seeing that the *kind* of reaction brought under notice in this paper is apparently unique, it is necessary to offer a few observations upon it.

$$2\cdot\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}+\left.\begin{matrix}Na\\Na\end{matrix}\right\}=2\cdot\left.\begin{matrix}Na\\C_4H_5\end{matrix}\right\}+\left.\begin{matrix}Zn\\Zn\end{matrix}\right.$$

The reaction here formulated may be regarded as an electrolytic decomposition—as an ordinary case of precipitation of one metal by a more electro-positive metal. Here ethyle is the electro-negative, and zinc the electro-positive member: sodium is more electro-positive than zinc, and accordingly sodium displaces zinc.

Following out the hypothesis—where the organo-metallic body

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contains a metal less electro-positive than the hydrocarbon radical, I should expect that the hydrocarbon radical would be eliminated by the action of sodium. Kakodyle, for instance, should give methyle and arsenide of sodium.

$$\left.\begin{matrix}C_2H_3\\C_2H_3\end{matrix}\right\}As+\left.\begin{matrix}Na\\Na\end{matrix}\right\}=\left.\begin{matrix}Na\\Na\end{matrix}\right\}As+\left.\begin{matrix}C_2H_3\\C_2H_3\end{matrix}\right\}$$

A case in point is afforded by the reaction of the alkali-metals with ammonia.

$$2.\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}K\\K\end{matrix}\right\}=2.\left.\begin{matrix}K\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}H\\H\end{matrix}\right\}$$

Of the same kind is the reaction of zinc-ethyle upon ammonia*.

$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=\left.\begin{matrix}Zn\\H\\H\end{matrix}\right\}N+\left.\begin{matrix}H\\C_4H_5\end{matrix}\right\}$$

To develope the hypothesis still further: just as the positive side admits of displacement by a more electro-positive radical, so should the negative side admit of displacement by a more electro-negative body.

The ordinary reactions of zinc-ethyle may be looked upon as illustrating this proposition, and can be written so as to exhibit a double displacement.

$$\overset{+}{Zn},\ \overset{-}{C_4H_5}+\overset{+}{I}\overset{-}{I}=\overset{+}{Zn}\overset{-}{I}+\overset{+}{C_4H_5}\overset{-}{I}$$

also
$$\overset{+}{Zn}\overset{-}{C_4H_5}+\overset{+}{O}\overset{-}{O}=\overset{+}{Zn}\overset{-}{O}+\overset{+}{C_4H_5}\overset{-}{O}$$

Inspection will show in all these cases, that an electro-positive radical displaces a less electro-positive radical; and an electro-negative radical displaces a less electro-negative one.

In accordance with the theory would be the displacement in sodium-ethyle of the ethyle by mercury, or by copper, &c., platinum, &c.

$$\begin{matrix}Na,\ C_4H_5\\Na\ C_4H_5\end{matrix}+\begin{matrix}Cn\\Cn\end{matrix}=\begin{matrix}NaCn\\NaCn\end{matrix}+\begin{matrix}C_4H_5\\C_4H_5\end{matrix}$$

Also a like displacement by arsenic or by nitrogen would be according to theory.

Pushing the hypothesis to its furthest limits, I should say that sodium-ethyle is only in *equilibrium* with bodies whose respective electrical sides lie either both of them *within*, or both of them *without* the space lying between the electro-positive sodium and the electro-negative ethyle.

* See Frankland's paper, Trans. Royal Soc. 1857.

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GEOLOGICAL SOCIETY.

[Continued from p. 150.]

February 2nd, 1859.—Prof. J. Phillips, President, in the Chair.

The following communication was read:—

"On the mode of formation of Volcanic Cones and Craters." By G. Poulett Scrope, Esq., M.P., F.R.S., F.G.S.

The author commenced by saying that he should not have referred again to this subject, already briefly treated by him in a paper read to the Society in April 1856, had it not been that Baron Humboldt, in the recently published fourth volume of his 'Kosmos,' applies the whole weight of his great authority to the support of the theory of upheaval in contradistinction to eruption as the *vera causa* of volcanic cones and craters,—a theory which the author, with Sir Charles Lyell, M. Constant Prévost, and many others, believes to be not merely erroneous, but destructive of all clearness of apprehension as to the character of the subterranean forces, and the part which volcanic action has played in the structural arrangement of the earth's surface.

He showed, by reference to the works of Spallanzani, Dolomieu, Breislak, &c., that the early observers of volcanic rocks and phenomena, together with the unscientific world, looked upon volcanic cones and craters, whether large or small, as the result of volcanic eruptions; but that of late years a new doctrine had been propagated by MM. Humboldt, von Buch, Elie de Beaumont, and Dufrénoy, which denies altogether that volcanic mountains have been formed by the accumulation of erupted matters, and attributes them solely to a sudden "bubble-shaped swelling-up" of pre-existing horizontal strata,—the bubble sometimes bursting at top and then leaving its broken sides tilted up around a hollow (elevation-crater).

The author expressed his belief that this notion originated in Baron Humboldt's account of the eruption of Jorullo in 1759, in which (as the author showed in his work on volcanos of 1825) a great error had been committed,—the convexity of the Malpais and its five hills being simply a bulky bed of lava poured out on a flat plain from five ordinary cones of eruption, and the "hornitos" common "fumaroles" coated over with black mud produced from showers of volcanic ashes mixed with rain-water. But the idea of a "bladder-like swelling-up" of horizontal strata into volcanic hills being thus started by M. von Humboldt, it was further extended by M. von Buch; and hence arose the "elevation-crater" theory.

The author next proceeded to show the inconsistencies of the advocates of this theory, who disagree among themselves as to the extent to which they apply it,—MM. Humboldt, von Buch, and Dufrénoy asserting both Somma and Vesuvius, the Peak of Teneriffe, and all Etna, to be solely due to sudden upheaval, while M. de Beaumont declares Vesuvius, the Peak, and the upper cone of Etna to be the products of eruption only. Again, while, except M. Dufrénoy, all admit the minor cones and craters of Etna, Vesuvius, Lanzarote, and Central France to be eruptive, all declare the similar

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cones and craters of the Phlegræan fields to be due only to upheaval. They offer no reliable test by which upheaved can be distinguished from eruptive cones; or, when they attempt this, differ again from one another, and even from themselves. Thus, von Buch considers the extreme regularity of the slopes of Etna a proof of its upheaval. M. de Beaumont asserts regularity of outline to be the distinguishing feature of an eruptive cone, and yet declares the upper and the lower portions of Etna, which are its least symmetrical parts, to be of eruptive origin, and the intermediate cone, the slope of which is extremely regular, to have been upheaved! In respect to the tuff-cones and craters of the Phlegræan fields, the series from Somma to the Monte Nuovo is so evidently of similar character, that, to avoid classing the first as an eruption-cone, the upheavalists have been driven to deny that the Monte Nuovo itself was the product of eruption, and even to assert that it existed in the Roman era, and was only sprinkled with a few ashes by the eruption which, from all contemporary authorities, threw it up in two days of the year 1538! The author describes the circular anticlinal dip of the strata of the Monte Nuovo and other tuff-cones of the Campi Phlegræi as utterly inexplicable upon the theory of upheaval, while it is the natural result of the fall and accumulation of fragmentary materials projected upwards by eruptions.

He then disputes the truth of M. de Beaumont's dogma, that lava cannot consolidate into a solid bed upon a slope exceeding 5° or 6°, and shows, from numberless instances in Auvergne and the Vivarais, on Etna, Vesuvius, Teneriffe, &c., that bulky beds of lava have congealed on steep slopes,—in some cases, as for example in that of Jorullo itself, in the form of a massive promontory projecting far from the side of the cone from the crater of which it issued; in others, when liquidity was at the *minimum*, in that of a dome or bell (Bourbon, Puy de Dome, &c.). In regard to Etna, he leaves M. de Beaumont's misrepresentations of fact to be dealt with by Sir C. Lyell, only remarking that, on M. de Beaumont's own showing, the portion of Etna which he supposes to have been upheaved, is positively "encrusted with a coating of lavas."

The inapplicability of the elevation-theory to the Cantal, Mt. Dore, and Mezenc in France is then shown, inasmuch as, by M. de Beaumont's own admission, the angle of slope of their basaltic and trachytic beds is even less than that of the recent and acknowledged lava-flows in the same district. Finally, he asks what has become of the products of the repeated eruptions of volcanos, if they have *not* accumulated in the course of ages into the mountains which we find there, composed of irregular alternating beds of lava and conglomerate just such as we see to be erupted from the central orifices?

The author next shows that the upheavalists have no correct idea of the mode of formation of craters, which are not formed, as they assert, at one blow, by a single explosion, like the bursting of a bubble, or of a mine of gunpowder, but by the repetition of explosions or flashings of steam from the surface of ebullient lava within the volcanic vent (like those of a colossal Perkins's steam-mortar), con-

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tinued for weeks and months, or more, by which the mountain is often ultimately eviscerated, its summit and heart being blown into the air, and scattered in fragments or ashes around—*not* foundering into the cavity and remaining there as they represent. He instances the great crater of Vesuvius formed under his eyes in 1822 by explosions lasting twenty days; and judging from the quantity of fragmentary matter then ejected and falling around, comparing it with the far greater quantities thrown up occasionally by eruptive paroxysms in other quarters of the globe, he asserts his belief that in the latter cases craters may be, and are, formed, of several miles in diameter, nothing remaining of the whole mountain except the wreck of its base, as we see in Santorini, the Cirque of Teneriffe, and so many other circular cliff-ranges surrounding extinct or active volcanic vents. He expresses his astonishment that von Buch and Humboldt should have supposed Vesuvius to have "sprung up like a bubble in one day, just as we now see it," in the year 79 A.D., and not to have increased since; and shows that even within the last hundred years great changes have taken place in the form of that mountain, and that the relation of Pliny of the phenomena witnessed by him is inconsistent with the idea of upheaval, and demonstrative of the occurrence of an eruptive paroxysm by which the upper part of Somma was blown by degrees into the air, and the crater of the Atrio formed, in which the subsequent eruptions of eighteen centuries have raised up the cone of Vesuvius.

In recapitulation, the author declares that the characters of all volcanic mountains and rocks are simply and naturally to be accounted for by their eruptive origin, the lavas and fragmentary matters accumulating round the vent in forms determined in great degree by the more or less imperfect fluidity of the former, which, as in the case of some trachytic lavas, glassy or spongy, may and do congeal in domes or bulky masses immediately over, or in thick beds near the vent, or, as in that of some basaltic lavas, may flow over very moderate declivities, to great distances; and consequently that the upheaval- or elevation-crater-theory is a gratuitous assumption, unsupported by direct observation and contrary to the evidence of facts. He concludes by representing its continued acceptance to be discreditable to science, and an impediment to the progress of sound geology, inasmuch as false ideas of the bubble-like inflation, at one stroke, of such mountains as Etna or Chimborazo must seriously affect all our speculations on Geological Dynamics, and on the nature of the subterranean forces by which other mountain-ranges or continents are formed.

XXXV. *Intelligence and Miscellaneous Articles.*

ON THE CONSTITUTION OF TITANIFEROUS IRON ORES. BY PROF. RAMMELSBERG.

RAMMELSBERG has published an elaborate investigation of the titaniferous iron ores, the principal results of which are as follows:—

1. The greater number of the titaniferous iron ores, among them

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all the crystallized forms, consist of 1 equiv. of titanic acid and 1 equiv. of protoxide of iron (protoxide of manganese or magnesia).

2. Magnesia is an essential constituent of all these ores. In the crystallized mineral from Layton, the magnesia amounts to 14 per cent.

3. According to Mosander's theory, the titaniferous iron ores are either simply titanates of protoxide of iron, FeO, TiO^2, with isomorphous admixtures of titanate of magnesia, or mixtures of such with sesquioxide of iron, for the most part in simple proportions.

4. The theory of H. Rose, that these ores consist of isomorphous sesquioxides of titanium and iron, would require the assumption of a sesquioxide of magnesium.

5. The author prefers Mosander's theory for the present state of our knowledge.

6. In iserine, we find grains consisting of FeO, TiO^2 and $3\,Fe^2O^3, TiO^2$.

7. No titaniferous iron crystallizing in regular octahedrons is known. The dense masses or octahedral grains which contain titanium appear to be mixtures.

8. The crystallized magnetic iron ores contain no titanium; they consist of one atom of protoxide and one atom of sesquioxide.

9. All the Elba iron ore does not contain titanium; but all, like that from Vesuvius, contains magnesia and protoxide of iron.

10. The strongly magnetic octahedrons from Vesuvius, hitherto considered as a specular iron, which are accompanied by rhombohedrons of specular iron, contain in part large quantities of magnesia, and in part protoxide of iron. They consist either of magnetic iron which has been partially converted into sesquioxide of iron, as well as of the isomorphous combination $MgO\,FeO^3$; or, as is more probable, the two protoxides are isomorphous with sesquioxide of iron, which is itself dimorphous.—Poggendorff's *Annalen*, vol. civ. p. 497.

ON GUAYACANITE, A NEW MINERAL SPECIES FROM CHILI.

BY FREDERICK FIELD*.

I send you a specimen of a mineral from the Cordilleras of Chili, which appears to me highly interesting. It consists entirely of copper, arsenic, and sulphur, having the following composition—

Copper	48·50
Sulphur	31·82
Arsenic	19·14
Iron, silver	traces
	99·46

—and consequently has the following formula, $3\,Cu^2S + AsS^5$, and may be considered as a tribasic sulpharseniate of copper, like the artificial tribasic sulpharseniate of potassium, in which that metal is replaced by Cu^2. Hardness, 3·5 to 4. Spec. grav. 4·39.

You will see it resembles Tennantite, in which the arsenic takes the place of the iron,—a specimen of Tennantite having the following value: Cu, 48·2; As, 12·5; Fe, 9·0; S, 31·14. I have proposed the name Guayacanite for this new species, as the mineral was first brought to the large copper-smelting works of Guayacana.

* From a Letter to Prof. Dana in Silliman's American Journal for Jan. 1859.

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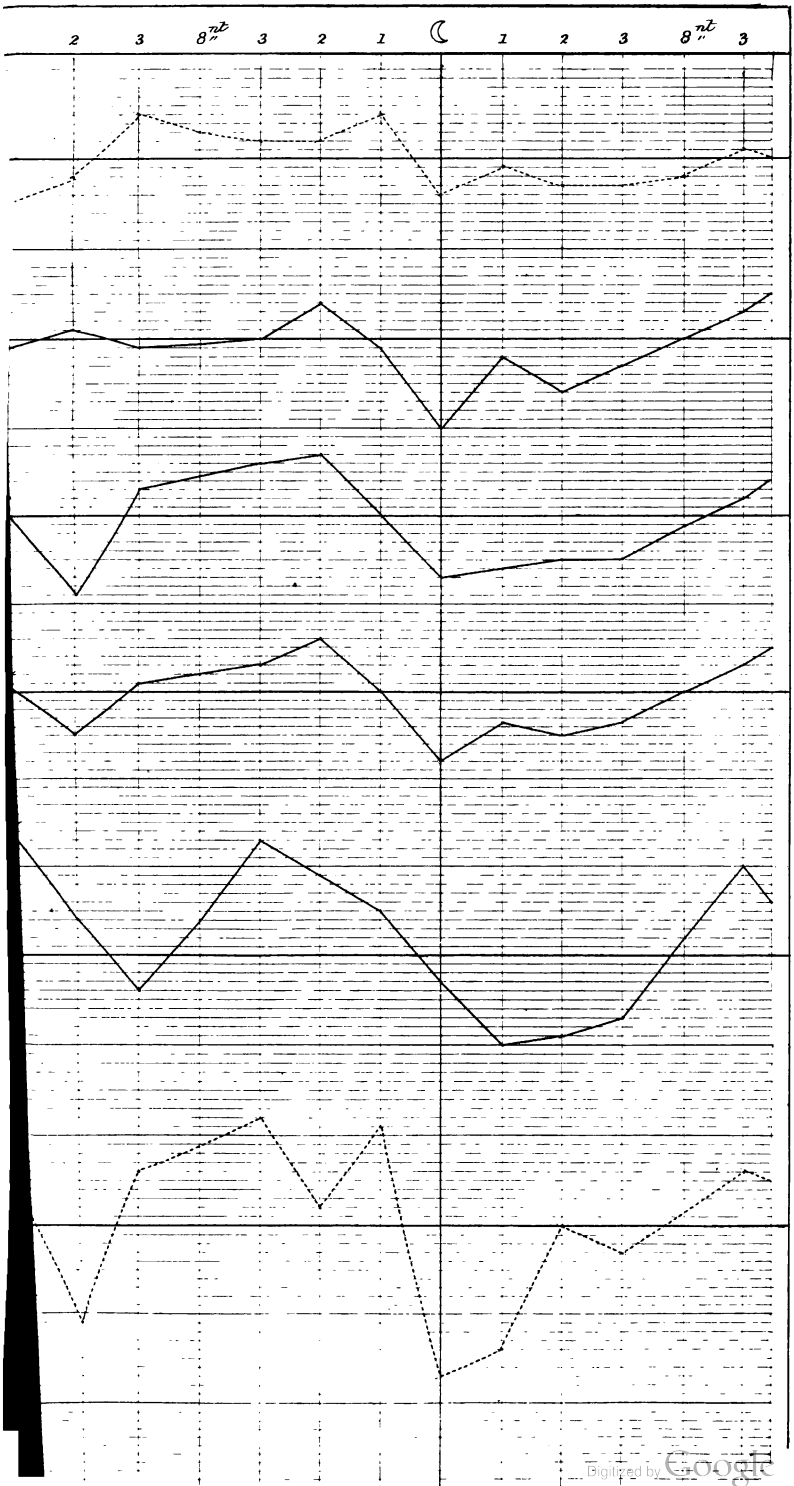
Cloud at Greenwich.



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[FOURTH SERIES.]

APRIL 1859.

XXXVI. *Investigations on the Thermal Effects of the Solar Spectrum.* *By* Dr. J. MÜLLER*.

Introduction.

AFTER the extension of the solar spectrum beyond the violet had become more fully recognized by means of fluorescence and photography, additional interest was attached to the extension of the spectrum in the opposite direction, that is, to the part which stretches beyond the red of the visible spectrum, and which can only be recognized by its thermal effects.

R. Franz (see Poggendorff's *Annalen*, vol. ci. p. 59) has lately published a summary of all the investigations which have been hitherto performed upon the thermal rays in the solar spectrum. It is therefore quite unnecessary for me to refer to the literature of the subject.

The researches of Melloni formed a new epoch in the doctrine of radiant heat. Numerous as were the discoveries which resulted from the application of the thermo-battery, and the introduction of the multiplier as thermoscope, nevertheless our acquaintance with the thermal relations of the solar spectrum was not greatly advanced, although Melloni had, in rock-salt, discovered a body which admitted the passage of all heat-rays alike.

Melloni's experiments on the warmth of the solar spectrum were almost exclusively confined to the determination of the position of the thermal maximum. In experimenting with a prism of rock-salt, he found that the maximum heating effect occurred at a point as far beyond the red boundary of the spectrum as the latter is distant from the point of transition of the

* Translated by Dr. F. Guthrie, from Poggendorff's *Annalen*, vol. cv. p. 337.

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green into blue (Pogg. *Ann.* vol. xxxv. p. 307). But neither in this nor in a subsequent paper are the numerical data furnished by the author. We find stated neither the magnitude of the deflections of his thermo-multiplier, nor the width of the spectra upon which he experimented. In short, in Melloni's memoir there are no fixed data whatever from which the curve of thermal intensity in the solar spectrum might be constructed.

At first Melloni regarded the rays of heat and light as essentially different; subsequently he pronounced his opinion distinctly as to their being completely identical.

The latter opinion, that rays of light and heat of the same refrangibility are absolutely identical, is also advocated by Masson and Jamin (*Comptes Rendus*, vol. xxxi. p. 14), who state that they found that all rays of heat within the visible spectrum are equally well transmitted by rock-salt, rock-crystal, alum, glass, water, &c., and that the unequal diathermancy of these substances wholly depends upon their possessing a different absorbent power for those dark rays of heat which are less broken than the red. There is probably no doubt as to the correctness of this result; still the above-mentioned physicists should not have withheld the experiments which they consider to have justified them in coming to this conclusion; for the mere enunciation of the result obtained, without the adduction of experimental evidence, can have little scientific value. If Masson and Jamin had published the observations which led them to the above conclusions, we should doubtless have been thereby provided with ample material to construct the curve of thermal intensity, at least within the visible spectrum.

To R. Franz belongs the merit of having been the first to publish data as to the amount of heat at different parts of the spectrum, the measurements being performed by means of the thermo-battery and the multiplier*.

Although every physicist who has made the thermal effects of the solar spectrum the subject of experimental investigation must have observed that, in a spectrum which is pure enough to show Fraunhofer's lines, the thermal effects are too small to admit of anything like exact measurement, yet Franz was the first to state this distinctly.

The experiments of Franz were performed with a flint-glass prism. The solar rays reflected from a metallic mirror entered the dark room through a slit of 4 millims. breadth. 32 millims. behind the first slit a second, 2 millims. in breadth, was placed, the prism being adjusted close behind the second slit.

At a distance of 17 centims. from the prism, the breadth of

* R. Franz, "Untersuchungen über die Diathermansie einiger gefärbten Flüssigkeiten" (Pogg. *Ann.* vol. ci. p. 46).

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the visible spectrum was about 18 millims. If we imagine the space bordering upon the red end of the spectrum to be divided into strips or zones of 3 millims. in width, then the first, second, third, &c. of these divisions is called by Franz the first, second, third, &c. dark zone. Franz found the following values for the thermal effects in the several divisions of the visible spectrum and in the dark zones:—

Fifth dark zone		0·83	Red . .	15·11
Fourth	„	3·01	Yellow. .	10·78
Third	„	6·11	Green . .	6·39
Second	„	8·77	Blue . .	3·61
First	„	11·87	Indigo. .	1·95
			Violet . .	0·85

The numbers are not the immediate values of the observed deflections of the multiplier, but are obtained by multiplying the mean value of the best observations by a constant factor, the numerical value of which is not given. It is much to be deplored that Franz did not give, at least in part, the direct deflections he observed; for it is from these alone that a measure can be obtained for the degree of accuracy of his results.

In consequence of glass absorbing a considerable quantity of dark thermal rays, a curve of thermal intensity constructed from the above numbers by no means represents the distribution of heat in a perfect thermal spectrum; knowledge of the latter can only be obtained by means of a prism of rock-salt.

Matters stood essentially in this position when I commenced my experiments upon the thermal effects of the solar spectrum: the results I have obtained furnish the material for the following pages.

Apparatus, &c. employed in the experiments upon the thermal spectrum.

Before proceeding to the separate series of experiments, it will be necessary to describe the apparatus I employed in performing them, and the manner in which the apparatus was arranged.

The thermo-battery was erected in a dark room arranged for experiments with solar rays.

Amongst the several multipliers at my disposal, one which I had myself constructed several years ago, in order to repeat Du Bois Reymond's experiments on the current in muscles, gave in combination with a thermo-battery of forty bismuth-antimony pairs, by far the largest deflections.

This multiplier consists of 3700 convolutions, which are, however, wound in four separate parcels: each parcel terminates in two screw-clamps, so that the four parcels may be employed

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either successively (in position) or side by side. The latter arrangement was found to cause a deflection more than twice as great as that caused by the former for equal radiations upon the thermo-battery. Hence in the following experiments the latter (side by side) arrangement was always adopted. Such a combination represents in reality a parcel of 925 convolutions of wire four times as thick as the single wire (0·3 millim. diameter). This multiplier was placed upon a bracket near the window of a neighbouring room. The current was conveyed from the thermo-battery to the multiplier and back through a 1 millim. thick copper wire covered with spun wool.

Before performing the separate series of experiments, this multiplier was graduated according to Melloni's method. Calling the strength of the current 1 which causes a deflection of 1° in the instrument, it was found that the deflection remained proportional to the strength of the current up to 20°; beyond this point, however, the following corresponding relations were found to exist between the deflection and the strength of the current.

Deflection.	Strength of current.	Difference of the strength of current	
		for 5°.	for 1°.
25	26		
30	32	6	1·2
35	40	8	1·6
40	52	12	2·4
45	69	17	3·4
50	92	23	4·6
55	122	30	6·0
60	162	40	8·0
65	223	61	12
70	323	100	20
75	510	187	37

In the experiments upon the thermal rays which are transmitted through coloured liquids, I made use of the thermo-battery already mentioned, consisting of forty bismuth-antimony couples. I am unable to give the origin of this battery, it having been in the Freiburg Physical Cabinet for a long time. For the experiments concerning the distribution of heat in the spectrum itself, I employed a lineal thermo-battery of fifteen couples especially procured for this purpose*. Masson and Jamin made use of a thermo-battery of exactly the same construction in their experiments on the heat of the solar spectrum.

* Procured from Lerebours et Secretan, Paris. See their Catalogue, No. 765. p. 93.

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In order that the reader may be enabled to compare the action of my instrument with that of others, I may here describe some experiments performed with this aim in view.

The flame (3 inches in height) of a Bunsen gas-jet, without the employment of a reflector, at a distance of 3 decimetres caused a deflection of 32° when radiating upon the quadratic thermo-battery, and a deflection of 11° when shining upon the lineal one—these deflections being shown by the multiplier before described.

A blackened copper-plate*, heated by a spirit-lamp at a distance of 2·5 decimetres, caused the multiplier to show deflections of 43° and 14° when radiating upon the quadratic and lineal thermo-batteries respectively. The solar rays with which I experimented were reflected into the darkened room, through a hole in the shutter of the window, from the metallic mirror of one of Silbermann's heliostats, which was set up outside.

Concerning the exactness of the following experiments, I have only further to add a few remarks.

The scale of the multiplier is only divided into intervals of five degrees; so that the readings off are only exact to $\frac{1}{2}$ a degree at furthest. A greater exactness in the reading off would, moreover, be wholly unreal; for, in the first place, the position of the needle of the multiplier was never a very definite one, after the source of radiant heat was removed, the variations being about $\frac{1}{2}$ a degree; secondly, the placing of the lineal thermo-battery in the spectrum was not capable of the same degree of accuracy as the reading off of the multiplier, because the spectrum was not pure enough to show Fraunhofer's lines.

It seems at present in vain to think of showing the existence of cold bands in the spectrum corresponding to Fraunhofer's dark lines; indeed, our knowledge of the thermal properties of the spectrum is still far behind that of its optical ones in respect to accuracy.

In the following pages the results of the observations are so given that the reader may judge of the limits of the errors of observation. I have in all cases avoided giving the results the appearance of an accuracy which they neither do, nor as yet probably can, possess.

Experiments on the thermal rays transmitted by coloured liquids.

When I first decided upon experimenting on the distribution of heat in the solar spectrum, I was not in possession of a lineal thermo-battery. I imagined it possible to dispense with one, and to arrive at the conclusions sought by endeavouring to determine the action of the thermal rays which had passed through

* See my 'Physics,' vol. ii. p. 614. 5th ed.

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coloured liquids. By determining, then, what portion of the visible spectrum is transmitted by the liquid under examination, I hoped to estimate the thermal action corresponding to this portion. It is, however, of course assumed here as completely established, that the heat- and light-rays are identical, in the sense in which this was supposed to be the case by Masson and Jamin.

The optical examination of the coloured liquids with which I proposed experimenting, was effected by throwing, by means of a flint-glass prism, a perfect solar spectrum upon a paper screen, introducing the liquid to be examined (contained between two glass plates 15 millims. apart) immediately before the slit through which the solar rays were admitted, and observing, finally, what part of the spectrum remained unextinguished.

In this manner the following results were obtained:—

1. A solution of cochineal allowed all the red rays, as far as the commencement of the orange, to pass through perfectly, so that the red of the spectrum was not weakened by the cochineal solution; but all the remaining colours of the spectrum were completely extinguished.

2. A solution of bichromate of potash allowed red, orange, and yellow to pass through almost entirely without absorption, together with a trace of green. The entire remainder of the spectrum was completely absorbed.

3. A solution of chloride of copper absorbs the whole of the spectrum except the green; but even the green was found to have diminished appreciably in brightness.

4. A solution of ammonio-sulphate of copper completely absorbed the least refrangible portion of the spectrum, while blue, indigo, and violet were transmitted without appreciable diminution in intensity.

In order to measure the power of these liquids in absorbing thermal rays, I first allowed the solar rays reflected from a metallic mirror to enter a dark room through a round orifice of 1 inch diameter. About $2\frac{1}{2}$ metres from this orifice the quadratic thermo-battery was placed, so as to be exactly in the middle of the incident pencil of rays. After reading off the corresponding deflection of the multiplier, a vessel, of the form described in my 'Physics' (5th ed., vol. i. p. 506), was placed before the opening, and being filled, first with water and then with the liquids mentioned before, in succession, the deflection caused in the multiplier was read off each time.

It was observed, however, that when the circuit through the thermo-battery was closed by the multiplier, a greater or less deflection was effected, even when all radiation upon the thermo-battery was prevented as much as possible. In addition, there-

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fore, to the deflection shown by the thermo-battery when subjected to the coloured rays, that deflection must also be taken into account which occurs before and after the experiment when the thermo-battery is protected as much as possible from all radiation. A series of experiments of this kind will render this clear:—

	Deflection.	Deflecting forces.
Free radiation	67	
Without radiation	−10	273
Pure water	56	140
Without radiation	−10	
Cochineal solution	36	52
Without radiation	−10	
Solution of bichromate of potash . .	48	91
Without radiation	− 8	
Solution of ammonio-sulphate of copper	6	13
Without radiation	− 5	

The "deflecting forces" given in the last column are easily calculated from the previous column of numbers. When the solar rays before falling upon the thermo-battery had to pass through pure water, the deflection was 56°. This deflection, according to the Table (page 236), corresponds with the strength of current 130. To this strength of current 10 has still to be added.

After these remarks, which sufficiently explain the process of the experiments, it will be sufficient so to arrange the three best series of experiments that the thermal effect of the rays which have passed through colourless water is denoted by 100.

	I.	II.	III.	
Colourless water . .	100	100	100	100
Red solution . . .	37	35	38	40
Yellow solution . .	65	64	70	74
Green solution . .	9	—	—	13
Blue solution . . .	9	9	9	13

The fact that the sum of the quantities of heat which pass through the yellow, the green, and the blue solution (*i. e.* 70+9+9=88) is not equal to the amount of heat which passes through colourless water, 100, evidently depends upon the absorption by each solution of some of the rays of its own colour: just as we already know that the green solution does not allow the passage of all the green rays of the spectrum. If we divide the difference, 100−88=12, in such a manner that 2 are given to red, 2 to orange and yellow, 4 to green and 4 to blue, indigo, and violet, then numbers expressing the warming power of the

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separate portions of the spectrum are obtained, which are shown in the fourth column of the foregoing table.

Accordingly the heating power

Of all violet, indigo, and blue rays of the spectrum would be 13
Of all green rays of the spectrum 13
Of all yellow and orange rays of the spectrum (74—40) . 34

Concerning the heating power of all the red rays of the spectrum, we cannot consider the number 40, corresponding to the red solution, as expressing this power immediately, because we are not justified from our experiments in assuming that the red solution allows only red, and not a certain amount of dark thermal rays, to pass through.

A red liquid, which transmits only red rays exclusively of the dark thermal rays, is probably as hypothetical as a yellow liquid which only allows the yellow and no red rays to pass through.

According to the experiments of Franz (Pogg. *Ann.* vol. ci. pp. 57, 58), we may assume that from 43 to 50 per cent. of all rays passing through the red solution are dark thermal rays. Hence, for the thermal power of all the red rays of the solar spectrum, we should have left the value 20 to 30.

From these experiments, it follows that the thermal power of the less refrangible rays of the solar spectrum, namely the red, orange, and yellow, is much greater than that of the green, blue, indigo, and violet. The numerical values given above, taken in conjunction with the extension of the separate colours in the spectrum, would lead to the construction of a curve of the intensity of the thermal force in the solar spectrum. We may, however, put this on one side; for the results obtained could not fail to be less exact than the one derived from a direct examination of the solar spectrum in its thermal relations.

Experiments on the distribution of heat in the spectra of a glass and of a rock-salt prism.

After completing the series of experiments described above, I proceeded to direct experiments on the distribution of heat in the solar spectrum, having obtained a lineal thermo-battery. A solar spectrum in which none of the thermal rays are absorbed can only be obtained, as is well known, by means of a prism of rock-salt. Before employing such a one, which I had got from J. V. Albert Sohn in Frankfort, I thought it well to experiment at first with a glass prism, in order to find out the best arrangement of the experiments, and to acquire the necessary practice in performing them, before removing the rock-salt prism from the glass case in which it was cemented.

After some preliminary experiments, the following arrangement of the apparatus was adopted:—

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The rays reflected from the metallic mirror of a heliostat entered the dark room through a slit 3 millims. in width. About 3 inches from the slit, a crown-glass prism was so placed that the transmitted rays underwent about the minimum deflection. A piece of tinfoil was glued upon the anterior face (that nearest the light) of this prism; by removing a strip of this foil 3 millims. in width, a second slit was produced immediately before the prism. Of course the prism was properly cleaned from glue at the part whence the tinfoil had been removed.

The lineal thermo-battery was placed at such a distance from the prism, that the visible spectrum had at that place a width of 18 millims.; this is the same spectral width as in Franz's experiments. The thermo-battery was fastened upon the brass rail of Melloni's apparatus, which stood at right angles to the pencil of rays emergent from the prism. The rail, upon which the thermo-battery could be easily moved and fixed, was divided into Paris lines.

A commutator was introduced in the connexion between the thermo-battery and the multiplier, and the latter was read off before, during, and after the thermo-battery was subjected to the radiation of any especial family of rays from the prism.

An example may render this course of observation more clear.

The thermo-battery was brought into the red of the spectrum; the solar rays which penetrated through the first slit were then received upon a pasteboard screen; that is, the thermo-battery was protected from radiation. On closing the commutator, the needle of the multiplier stood at

$$+3^{\circ}\cdot5.$$

The screen protecting the prism was now withdrawn, so that the thermo-battery received red rays; the needle of the multiplier stood at

$$-7^{\circ}.$$

On again intercepting the incident rays, so that the thermo-battery was again protected from radiation, the needle of the multiplier stood again at

$$+3^{\circ}\cdot5.$$

Now the conclusion hence to be drawn is, that the radiation of the red rays upon the thermo-battery had in this instance effected a deflection of $10^{\circ}\cdot5$.

The commutator was now reversed, and the observation repeated in the same manner: it was now found that the radiation of the red rays on the thermo-battery caused a deflection of $9^{\circ}\cdot5$. The mean value, therefore, of the thermal action produced by the incidence of the red rays upon the thermo-battery, corresponds to a deflection of 10° of the needle of the multiplier.

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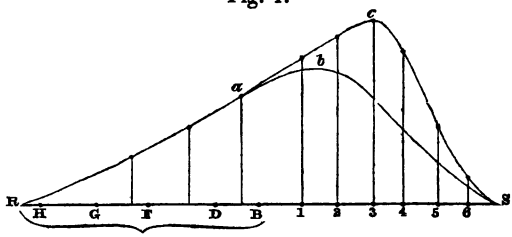
In the following Table the mean values are given, for the values estimated in the above manner, of the thermal effects in the different portions of the spectrum :—

	°
Boundary of indigo and violet . . .	2
Middle of the blue	4
Middle of the yellow	7
Middle of the red	10
$1'''$ beyond the red boundary	12
$2'''$ " "	11
$4'''$ " "	7
$6'''$ " "	2

According to these observations, therefore, the maximum temperature of the spectrum from crown-glass lies beyond the boundary of the red, while Franz found this maximum for his flint-glass prism to lie in the red itself. This difference, however, may be readily explained. The path which the rays had to traverse in the interior of Franz's prism was about 18 millims., while I had so adjusted the slit in the tinfoil coating that the path traversed by the rays in the glass was only 10 millims. Inasmuch now as the glass acts as an absorbent for the thermal rays, the thermal maximum must clearly undergo a displacement towards the red when the thickness of the glass to be passed through increases.

The curve of thermal intensity, R*ab*S (fig. 1), for the spectrum of a glass prism is constructed according to the above numerical data. We see in this figure that the thermal elongation of the spectrum beyond the red occupies a space almost as long as the whole of the visible spectrum. (The latter is marked by a bracket in the figure.) In the visible portion of the spectrum, the positions of the more important Fraunhofer's lines are marked for perspicuity.

Fig. 1.



According to the observations of Franz, the dark portion of the thermal spectrum is nearly the same length as I have observed it to be; indeed, our observations in general either agree perfectly with one another, or differ only to an immaterial extent. I considered it, therefore, unnecessary to institute a second series of experiments with the glass prism, and proceeded to employ the one of rock-salt.

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This prism was an equilateral one, each side being 36 millims. long. The prism from end to end was 44 millims. As it was impossible to stick tinfoil upon this prism without injuring the incident surface, a sheet of brass was employed instead, in which a slit 3 millims. wide was cut, and which was placed close before the incident surface of the rock-salt prism.

In the following Table the results of two series of experiments are given, each series having been performed in exactly the same manner as that described in speaking of the glass prism.

	I	II.
	°	°
Violet end	0·5	
In the blue	3·0	2·0
In the yellow . . .	5·5	4·8
In the red	7·3	6·0
$1'''$ in the invisible .	9·7	8·3
$3'''$ „ .	11·5	9·7
$4'''$ „ .	10·5	7·5
$6'''$ „ .	1·0	1·2

Taking the mean of the two numbers in each case, and multiplying it by a factor, which gives the product 10 for the red, we have,—

	°
In the blue	3·7
In the yellow	7·9
In the red	10·0
$1''$ in the invisible	13·2
$3'''$ „ „	15·9
$4'''$ „ „	13·2
$6'''$ „ „	1·7

The fact that the deflections for the visible portions of the spectrum in the case of the rock-salt prism were smaller than when the glass prism was employed, is clearly entirely owing to the fact that the surfaces of the rock-salt could not be so perfectly ground and polished as those of glass, and, further, to the existence of a slight turbidity in one part of the rock-salt prism.

It is true that these numbers do not quite correspond with those for glass, even in the visible portion of the spectrum; nevertheless the discrepancies, which certainly result wholly from errors of observation, are not of a kind to justify us in calling in question the identity of the curves of thermal intensity in the visible portions of the spectra derived from the glass and rock-salt prisms. (It is of course impossible here to take into account the differences of partial dispersion for glass and rock-salt.) Beyond the red boundary of the visible spectrum, however, the two curves of intensity separate widely from one

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another, as is seen in fig. 1, p. 242, in which the curve R *a c* S belongs to the spectrum of the rock-salt prism.

In this spectrum accordingly, the thermal maximum lies still further from the red than in the spectrum of the glass prism. And indeed (agreeably also to Melloni's results) the distance of this maximum from the boundary of the red is about as great as that of the transition of the green into blue from the red boundary of the spectrum.

The dark thermal extension of the spectrum, according to these experiments, is not greater for rock-salt than for glass.

As rock-salt allows all kinds of thermal rays to pass through equally well, the curve R *a c* S represents the true distribution of heat in a spectrum produced by refraction, and unmodified by partial absorption.

Estimation of the index of refraction, and of the undular length of the extreme dark thermal rays of the solar spectrum.

It has been established by the above experiments, that the dark thermal rays contained in the solar spectrum extend far beyond the red boundary of the visible spectrum, and that the Fraunhofer's line B (for a crown-glass spectrum) lies about midway between the violet end of the spectrum and its extreme dark thermal rays (fig. 1, p. 242). Now, for crown-glass, the index of refraction for H is about 1·546, and for B about 1·526: hence it follows that the refractive index of the extreme dark thermal rays of the solar spectrum is about 1·506.

It is clearly impossible to determine the undular length of the dark thermal rays of the solar spectrum, by employing the same method as that used in finding the undular length of the luminous rays of different colours. We must, on the contrary, assume the undular lengths of the differently-coloured luminous rays as known, and deduce the undular length of the extreme dark thermal rays from their indices of refraction, by making use of the subsisting relation between index of refraction and undular length.

The results of my experiments appear to disagree with Cauchy's formula of dispersion, which is said to connect undular length and refractive index. I purpose discussing this subject more particularly on a subsequent occasion.

I propose at present to endeavour to establish the connexion between undular length and refractive index by means of an empirical formula. Let us denote the undular length by w, the refractive index by e, we may then put

$$w = a + b\,e + c\,e^2 \ldots\ldots\ldots \quad (1)$$

If, in this equation, we put in succession three corresponding

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values for w and e, namely those belonging to the Fraunhofer's line B,

$$w=690, \qquad e=1{\cdot}526;$$

secondly, those belonging to the Fraunhofer's line F,

$$w=485, \qquad e=1{\cdot}536;$$

and finally, those belonging to the Fraunhofer's line H,

$$w=396, \qquad e=1{\cdot}546$$

(multiplying the real value of w with 1000000, in order to avoid the decimal places), we obtain three equations, from which the following values of a, b, and c are derived:—

$$a= \quad 1391460,$$
$$b=-1796460,$$
$$c= \quad 580000.$$

Substituting these numbers in equation (1), we get

$$w=1391460-1796460e+580000e^2. \quad . \quad . \quad (2)$$

From this equation the undular length of the extreme thermal rays of the solar spectrum is obtained by substituting for e the index of refraction 1·506: we get then

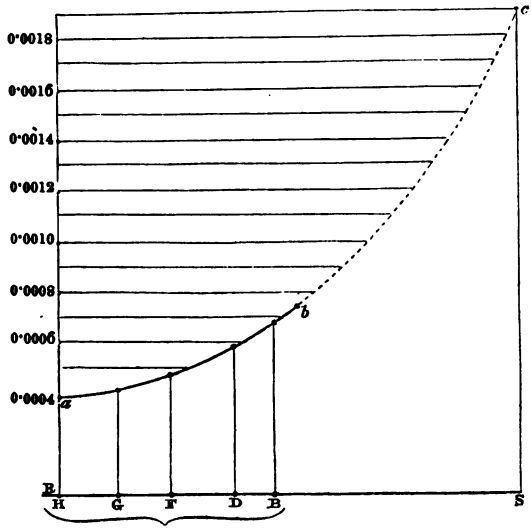
$$w=1770;$$

or, rather, redividing by 1000000,

$$w=0{\cdot}00177.$$

The same result may be obtained more easily and clearly in a graphic manner. In fig. 2, the line R S represents the

Fig. 2.



solar spectrum produced by a prism of crown-glass on the same

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scale as fig. 1, p. 242. From the points H, G, F, D and B, which correspond to the Fraunhofer's lines occurring at these points, perpendiculars are drawn whose lengths are proportional to the undular lengths. This proportion is such that a difference of 5 millims. in height corresponds to a difference of 0·0001 millim. in the undular length. The curve *a b*, drawn through the extremities of the perpendiculars H, G, F, D and B, represents, then, the law connecting the undular length with the refractive index in crown-glass.

The curve *a b* is continued beyond *b* in such a manner that the course of the continued portion *b c* joins in as continuous a manner as possible the portion *a b* which has been derived from observations; in other words, the law, according to which the undular length and refractive index are connected in the visible portion of the spectrum, is preserved in the graphic continuation of the curve into the ultra red dark thermal rays.

If, now, at S, that is, at the point which corresponds with the extreme limits of the dark thermal rays of the solar spectrum, a perpendicular be erected, this will cut the curve in a point C, whose height above the axis of abscissæ R S corresponds to an undular length 0·0019; that is, the undular length of the extreme dark thermal rays of the solar spectrum is

0·0019 millimetre.

This value cannot of course be considered as an exact, but only as an approximate one, because the extension of any empirical law beyond the limits of the observations upon which it is based can never lay claim to any great degree of exactitude, whether such extension be performed algebraically, or, as in this case, graphically.

The mean of the two values, the one 0·00177, obtained by calculation, the other 0·0019, graphically gives us

$$w = 0{\cdot}00183 \text{ millim.}$$

for the undular length of the extreme dark rays of the solar spectrum.

The undular length of the extremest fluorescent rays derived from sunlight, is, according to Esselbach*, 0·0003 millim. The next lower octave to these rays, which are the most refrangible of all, gives the undular length 0·0006 millim., which, as is seen from fig. 2, nearly corresponds to the Fraunhofer's band D in the orange.

The second lower octave of the most refrangible rays, with the undular length 0·0012 millim., falls in the midst of the dark thermal rays of the solar spectrum. The third lower octave, with

* See my 'Physics,' 5th ed. vol. i. p. 698; also Pogg. *Ann.* xcviii. p. 513.

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the undular length 0·0024, falls beyond the boundary S of the solar spectrum, which corresponds to the least refrangibility. *Altogether, therefore, the solar spectrum contains rather more than* $2\frac{1}{2}$ *octaves, that is, rays whose undular length is between* 0·0003 *and* 0·0018 *millimetre.*

Distribution of heat in the diffraction-spectrum.

It is well known that the distribution of the colours in a "grid"-spectrum are quite differently arranged from those of a prismatic spectrum. It may therefore be predicted that the thermal curve of intensity for a grid-spectrum would have quite a different form from that given in fig. 1, which is the curve for a prismatic spectrum.

As far as I am aware, Draper was the first who examined the heat in a diffraction-spectrum. He experimented upon a grid-spectrum obtained by reflexion*. The manner in which he conducted his experiments is exceedingly imperfectly described; nor does he give the slightest account of the magnitude of the effects which he obtained by the multiplier. He only asserts that he found the maximum heat in the yellow. Although, as he himself admits, his results are "imperfect and incomplete," still a more exact description of his apparatus, the arrangement of his experiments and conclusions, would have been of interest to physicists.

If I understand rightly, Draper employs as thermo-battery a single thermo-electrical element. If this be so, it is no wonder that the thermal effects which he observed were, as he himself admits, exceedingly small. Indeed, according to my own experience, I can scarcely comprehend how he could observe more than traces of such effects.

Being in possession of an excellent "grid," similar to that with which Eisenlohr experimented†, and for which I am indebted to the kindness of Professor Schwerd, I commenced experiments on the radiant heat in the diffraction-spectrum. I found, however, although employing a thermo-battery of fifteen pairs, that the thermal effects were so small that I relinquished the hope of obtaining useful results in this manner; for appreciable deflections of the multiplier could only be obtained by bringing the thermo-battery so close to the grid that the two spectra were so impure that they were completely confounded.

The small amount of heat in the diffraction-spectrum is easily explained. If no lens be employed, not more than 2 millims. broad of the grid can be used, if anything like pure

* Phil. Mag. 1857, vol. xiii. p. 153.
† Pogg. *Ann.* vol. xcviii. p. 354.

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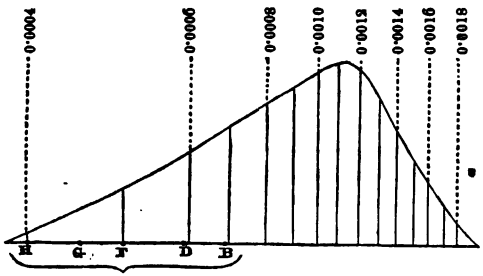
spectra are desired. On bringing the lineal thermo-battery (rather more than 2 millims. in width) into the place of the grid, the total action of the pencil of rays falling directly upon the thermo-battery was a deflection of the multiplier of 30°. If, now, the same quantity of rays fall upon a portion of the grid 2 millims. in thickness, about half of the rays are intercepted by the opake portions of the grid; consequently a total thermal action of 15° remains. This heat, however, is distributed among a whole series of spectra. If, neglecting the middle image, we only suppose there to be seven such spectra, scarcely 2° remain for the total thermal action of a single refraction-spectrum, even neglecting all other sources of loss. But if the total effect of such a spectrum is only 2°, it is impossible to expect noticeable thermal effects in its separate portions.

Although experimental means for accomplishing this purpose are not yet so complete as to admit the derivation of the course of the thermal curve in the diffraction-spectrum by means of direct experiments, still it is possible to get at the same result in an indirect manner. The course of the thermal curve in the diffraction-spectrum may be derived from that of the thermal curve obtained (in fig. 1, p. 242) for the refraction-spectrum, most simply in the following manner :—

In fig. 2, p. 245, let us imagine perpendiculars drawn to the line R S from the points in which the horizontal lines cut the curve *a b c*. These perpendiculars will divide the whole length of the spectrum into fifteen divisions, each of which corresponds to a difference of 0·0001 millim. in the undular length; that is to say, passing from left to right, every successive vertical corresponds to an undular length greater by 0·0001 millim. than the preceding one. Fig. 3 represents the curve of thermal

Fig. 3.



intensity for the refraction-spectrum, together with these verticals.

If, now, a diffraction-spectrum, of a width H S, equal to that of the refraction-spectrum (fig. 3), has to be divided into fifteen parts, each of which corresponds to an increase of 0·0001 millim.

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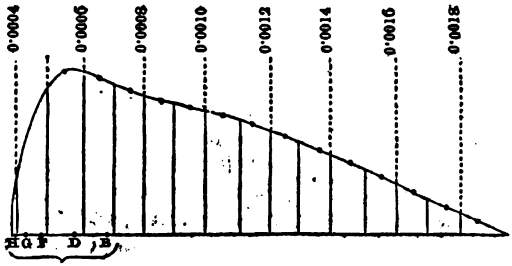
in the undular length, then the whole length H S has to be divided into fifteen *equal* parts, as is done in fig. 4. The corre-

Fig. 4.



sponding divisions of the refraction- and deflection-spectra are accordingly of unequal width. The same quantity of rays is distributed in the one over a greater or less space than in the other; whence for corresponding places of the two spectra the heating must appear unequal.

The space from the violet to the boundary of the blue and green, between the verticals 0·0004 and 0·0005, occupies a width of 22 millims. in the prismatic spectrum (fig. 3, p. 248), and only a width of 6 millims. in the grid-spectrum. Hence in the grid-spectrum the violet, indigo, and blue rays are compressed into $\frac{3}{11}$ of the space which they occupy in the prismatic spectrum. At the place in question, therefore, the intensity of the heat must be $\frac{11}{5}$ times as great in the diffraction-spectrum as in the refraction-spectrum (fig. 3), and accordingly the ordinate in the middle, between the verticals 0·0004 and 0·0005 in fig. 4, is made $\frac{11}{5}$ times as great as the height of the ordinate in the middle between 0·0004 and 0·0005 in fig. 3.

The division between the verticals 0·0005 and 0·0006 (containing chiefly green and yellow) is 12 millims. broad in fig. 3, and 6 millims. in fig. 4, that is, only half as broad in the latter. As the ordinate in the middle between 0·0005 and 0·0006 for fig. 3 is 16 millims. in height, the corresponding ordinate in fig. 4 must be twice as high, that is, 32 millims., if it is proportional to the heat which occurs at this point.

Proceeding in the same manner, points have been determined through which the curve of intensity (fig. 4) has been drawn. This has been done by making the middle of each of the divisions in fig. 4 as much higher or lower than the corresponding ordinate in fig. 3, as the breadth of the division in fig. 4 is less or greater than the breadth of the corresponding division in fig. 3.

Although the curve (fig. 4) cannot lay claim to great accu-

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racy, still it represents the essential distribution of heat in the diffraction-spectrum. Draper's assertion, that the maximum heat falls in the yellow, is certainly thereby supported; but it by no means follows thence, that "*the distribution of heat corresponds to the distribution of light*," as Draper asserts. Draper, indeed, entirely ignores the dark portion of the thermal spectrum, appearing to imagine that the intensity of heat, like that of light, diminishes equally towards the violet and red boundaries of the visible spectrum.

Because, in the refraction-spectrum, the curve of luminous intensity differs so entirely from the course of the thermal curve, the two curves cannot coincide in the diffraction-spectrum, although in the latter the luminous and thermal maxima approach one another incomparably more closely than in the refraction-spectrum.

We see from fig. 4, p. 249, that the dark thermal rays in the diffraction-spectrum occupy a space which is about $3\frac{1}{2}$ times as broad as the whole visible spectrum. In the diffraction image formed by a grid*, the dark part of the first thermal spectrum extends accordingly on both sides as far as the violet of the third light spectrum, that is, from R to V'. Hence almost the whole of the second light spectrum is covered by the dark thermal rays of the first spectrum,—a circumstance which, putting aside other difficulties, renders it impossible to trace directly the curve of intensity of a single diffraction-spectrum.

XXXVII. *On the Conservation of Energy.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE extract from a recent work of Dr. Faraday, which you have reprinted in your Magazine for March, has suggested to me the following remarks:—

1. It is certain that no law of "conservation" is applicable to the *tendency of a body to change its place*, nor to any mere *tendency* whatsoever.

2. The quantity whose amount is "conserved" during all the mutual actions amongst a system of bodies, is always the *product of two factors*; and when one of those factors is the magnitude of a *tendency* towards change of a particular kind, the other factor is the magnitude of the *change throughout which that tendency is capable of continuing to act.*

When the magnitude of the tendency is variable, it is of

* See my 'Physics,' ed. 5, vol. i. p. 622, fig. 699.

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course to be understood that the *mean* value of that magnitude is the factor to be used in the multiplication; or, what is the same thing in other words, that the *integral of the tendency with respect to the change* is the quantity in question.

When applied to the mechanics of bodies moving uniformly, the conservation of the quantity now described means neither more nor less than the old principle of "virtual velocities."

3. Inasmuch as the word "force" has for a long time been used to denote a *tendency* of a pair of bodies to change of relative motion, a kind of magnitude to which no law of conservation applies, the use of the phrase "conservation of force" gives rise to misapprehensions and groundless disputes, by implying a different meaning of the word "force;" and even although that phrase has been sanctioned and widely spread by Mr. Grove's celebrated work, it is much to be desired that a phrase should be adopted in its stead about which there is no ambiguity, such as "conservation of energy." The only precisely-defined meaning which has ever been assigned to the word "energy" in writings on physical science, is that which has been described in Article 2 of this letter. I believe that the first definition of "energy" in this sense is due to Young (Lectures on Natural Philosophy, Lecture VIII.).

4. Inasmuch as a mass moving with a given velocity must have been acted upon during a certain time by a force (in the sense of *tendency*) represented by

$$\frac{\text{mass} \times \text{velocity}}{\text{time of action}},$$

and through a distance equal to

$$\text{half-velocity} \times \text{time of action};$$

and inasmuch the same mass in the course of having its motion stopped, is capable of overcoming a resisting force as stated above through a distance as stated above, the energy of a moving mass is the *product of its mass into the half-square of its velocity.*

5. In order to distinguish from each other the two forms of *mechanical* energy, as well as other forms of physical energy which are analogous to them, I proposed, in 1853 (Proc. of the Phil. Soc. of Glasgow, 1853, and Edinb. Phil. Journ. 1855), to distinguish as *potential energy* that which consists in a tendency towards a change capable of continuing to act throughout a given change; and as *actual energy*, that which consists in a state of change going on, such as the motion of a mass with a given velocity. At the time, I supposed those terms to be wholly original (as I believe they were, in their application to forms of energy other than mechanical); but I have since found that Carnot, in his essay on the principles of equilibrium and

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motion, proposed a pair of analogous terms in the case of mechanical energy, viz. "virtual *vis viva*" and "actual *vis viva*."

6. The principle of the conservation of energy may be thus stated:—*In any system of bodies, the sum of the potential and actual energies of the bodies is never altered by their mutual actions.*

When applied to phænomena purely mechanical, this is neither more nor less than the long-known law of the conservation of *vis viva*. The later discoveries respecting its applications have reference to those cases in which the law of the conservation of *vis viva* fails, and in which the increase or diminution of energy in the mechanical forms is compensated for by the diminution or increase of energy in other forms; such as energy of heat, which is the product of weight × temperature × specific heat × Joule's equivalent; energy of electric current, which is proportional to electromotive force × quantity of current, or, otherwise expressed, to (quantity of current)^2 × resistance of circuit, &c.

7. In the case of GRAVITATION, the quantity which varies inversely as the square of the distance between a pair of bodies, is their *tendency* to approach each other, to which no law of conservation applies.

8. To find their *energy*, being the quantity which *is conserved*, the following processes are to be gone through.

To fix the ideas, let the bodies be spherical; let a denote the sum of their radii, and m, m' their respective masses: let their *tendency* to approach each other, when their surfaces are in contact and their centres at the distance a apart, be denoted by $\frac{\mathrm{F}mm'}{a^2}$. Then, when their centres are at any other distance apart, r, their tendency to approach each other is

$$f = \frac{\mathrm{F}mm'}{r^2}.$$

At any given instant, let r_1 be the distance between the centres of the bodies, and let their velocities, referred to their common centre of gravity as a fixed point, be v, v'. (It is well known that those velocities are contrary in direction, and inversely as the masses of the bodies.) Then

I. To find the potential energy, construct a curve whose abscissæ are values of the distance r, and its ordinates values of the attraction f, and take the area of that curve between the ordinates whose abscissæ are a and r_1, that is to say, in symbols, *the potential energy*

$$= \int_a^{r_1} f dr = \mathrm{F}mm' \left(\frac{1}{a} - \frac{1}{r_1} \right).$$

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This may be otherwise expressed by saying that the potential energy is the product of the *distance* between the surfaces of the spheres, $r_1 - a$, into the *mean attraction* through that distance, $\frac{Fmm'}{ar_1}$.

II. *The actual energy*

$$= \frac{mv^2}{2} + \frac{m'v'^2}{2}.$$

III. *The total energy*

$$= Fmm'\left(\frac{1}{a} - \frac{1}{r_1}\right) + \frac{mv^2}{2} + \frac{m'v'^2}{2};$$

and this quantity is *conserved*, being incapable of change by the attractive force exerted between the two bodies, whatever variations the distance r_1 may undergo.

This is nothing more than what is demonstrated by Newton in propositions 39 and 40 of the first book of the 'Principia.'

The quantity above denoted by $\frac{Fmm'}{r_1}$ is well known to mathematicians under the name of "potential function."

9. Besides energy, there are two mechanical magnitudes which are constant in a system of bodies acted upon by their mutual forces only; viz. the *resultant momentum* of the system, and its *resultant angular momentum*: but these quantities are purely mechanical, and their conservation in the mechanical form is absolute under all circumstances, being a necessary consequence of the equality of action and reaction; so that they do not connect mechanics with other branches of physics, as the quantity called energy does*.

10. The principle which the preceding remarks are intended to illustrate may be summed up by saying that *energy, or the quantity which remains constant in all physical actions amongst a system of bodies, is either the product of two factors—a tendency or effort to produce a change, and the change throughout which that effort is capable of continuing to act,—or is equivalent to such a product*; and that, consequently, no law of conservation is to be looked for when one factor only of that quantity is considered; such, for example, as the attraction between two bodies statically measured.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, March 12, 1859. W. J. Macquorn Rankine.

* In the case of a pair of attracting bodies, the *resultant momentum* relatively to their common centre of gravity is always *nothing*. The *resultant angular momentum* is the sum of the products made by multiplying each mass into twice the area swept over by its radius-vector in unity of time; and is constant, according to one of "Kepler's laws."

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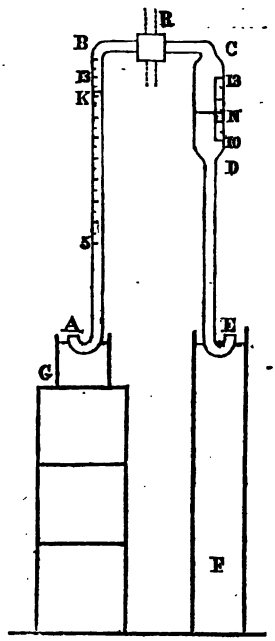
XXXVIII. *On a Method of determining the Specific Gravity of Liquids.* By Thomas Tate, *Esq.**

THIS method of determining the specific gravity of liquids depends on the principle, that the lengths of the columns of two different liquids, supported by the same atmospheric pressure, are to each other inversely as their specific gravities.

A B C E represents a glass tube, with an enlargement at C D, bent somewhat in the form of the Würtemberg siphon, and fixed at R to the rod of a retort-stand; E F a deep jar filled with distilled water; A G a small glass vessel containing the liquid whose specific gravity is to be determined. The tubes A B and D C are graduated into inches and tenths, the divisions on the former being measured from the top of the tube A, and on the latter from the top of the tube E. The length of A B or E C may be about 13 inches, and C D about $3\frac{1}{2}$ inches; the diameter of the portions A B and E D may be $\frac{1}{2}$ an inch, and that of the portion C D about 1 inch. The jar E F should be deep enough to submerge the tube E C. The tops of the tubes at A and E should be plane sections at right angles to the axes of the tubes. The instrument is used in the following manner:—



The glass G, being at first empty, is raised by a thin block of wood, causing the bent tube A to touch the bottom of the glass; the jar E F, nearly filled with distilled water, is elevated until the water stands at C, the top of the wide tube D C: whilst the jar is held in this position, the liquid, whose specific gravity is to be found, is poured into the glass G; the jar is then let down to its original position, leaving the mouth of the tube E a little above the level of the water in the jar (the water in the tube D C will have fallen a little, whilst the liquid will have risen in the tube A B); the glass G is then depressed by removing the thin block of wood, thereby causing the mouth of the tube A to stand a little above the level of the liquid in the

* Communicated by the Author.

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glass. Now the water will stand exactly at the top of the tube E, and the liquid exactly at the top of the tube A; and as the two columns of liquids are supported by the same atmospheric pressure, the specific gravities or densities of the two liquids will be to each other inversely as the heights of their respective columns. The heights of the respective columns being read off to inches and tenths of inches, the quotient arising from the division of the length of the column of water by the length of the column of liquid will give the specific gravity of the liquid at the temperature of observation, that of water being unity. The specific gravity of the liquid may thus be readily found true to three places of figures, and, by using a hand vernier, to four places of figures. The allowance necessary to be made for the curvature due to cohesion, as well as the method of reducing the specific gravities to mean temperature, are too well known to require any explanation in this place.

It will be observed that it is not at all requisite that the fluids at A and E should stand at the same level; and it will be further observed, that the truth of the indications is not at all vitiated by the pressure of the vapour which may arise from the liquid in the tube A B, inasmuch as this additional pressure would act equally on both liquids. As the specific gravity of the liquid depends solely upon the relative heights of the columns A K and E N, it matters not what quantity of liquid may have entered the tube. After the observation has been made, the liquid may be discharged from the tube, A B, by simply elevating the jar F.

In order to economize, when desirable, the quantity of liquid employed, the following artifice may be adopted:—In the place of pouring the liquid into the glass G, let it be poured gradually into the mouth of the tube as required, whilst the jar F is being slowly depressed. Proceeding in this way, about an ounce weight of the liquid will be sufficient for determining its specific gravity.

The bendings at A and E are not strictly essential to the construction of the instrument, being introduced rather to facilitate the manipulation than to contribute to the delicacy of the indications. When the tubes are straight at their lower extremities, two glass tubular gauges may be used, terminating at their lower extremities with fine points, and sliding through rings of india-rubber fixed at the upper portions of the tubes B and C. In this case the graduated tubes are depressed until their fine points just touch the surfaces of the liquids in the vessels F and G, and then the heights of the respective columns are read off as before.

I have used this instrument for some time, and compared the

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results derived from it with those obtained by means of a common areometer of the best construction, and I have invariably found that they closely agree with each other.

Let h = EN, the height of the column of water;
h_1 = AK, the corresponding height of the column of liquid;
P = the pressure of the atmosphere, expressed in inches of mercury, at the time of observation;
p = that of the air, KBCN, enclosed in the tube;
s_1 = the specific gravity of the liquid, s being that of water, and S that of mercury; then we have

$$P = p + h \times \frac{s}{S},$$

and also

$$P = p + h_1 \times \frac{s_1}{S}, \quad \ldots\ldots\ldots \quad (1)$$

$$\therefore s_1 = \frac{h}{h_1} \times s;$$

and taking $s = 1$,

$$s_1 = \frac{h}{h_1}, \quad \ldots\ldots\ldots \quad (2)$$

that is, the specific gravity of the liquid is equal to the quotient arising from the division of the length of the column of the water by that of the column of the liquid.

Suppose the liquid whose specific gravity is to be determined, to be rectified alcohol, then in this case I have found $h = 10{\cdot}25$ and $h_1 = 12{\cdot}20$; $\therefore s_1 = \frac{10{\cdot}25}{12{\cdot}20} = {\cdot}8401$.

In order to determine the proportion of the parts of the instrument corresponding to the most economic form of construction, let

a = the section of the wide tube CD;
a_1 = the section of the tube AB; H = AB = EC;
V = ea_1 = the volume of the air in the tube at P pressure, that is, when the water stands at the level C;
v = the volume of the air, KBCN, at p pressure;

then we find

$$PV = pv; \quad v = V - a_1 h_1 + a(H - h);$$

and by equation (1),

$$p = P - h_1 \frac{s_1}{S};$$

$$\therefore PV = \left(P - h_1 \frac{s_1}{S}\right)\left\{V - a_1 h_1 + a(H - h)\right\}.$$

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Solving this equality for the value of $\frac{a}{a_1}$, substituting $h_1 s_1$ for h, and putting $\left(\frac{d}{d_1}\right)^2$ for $\frac{a}{a_1}$, we obtain

$$\frac{d}{d_1} = \sqrt{\frac{h_1}{\mathrm{H} - h_1 s_1}\left(\frac{e s_1}{\mathrm{PS} - h_1 s_1} + 1\right)}. \quad . \quad . \quad (3)$$

But since $e s_1$ is small, in all actual cases, as compared with $\mathrm{PS} - h_1 s_1$, we have approximately

$$\frac{d}{d_1} = \sqrt{\frac{h_1}{\mathrm{H} - h_1 s_1}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now assuming s_1 to be the least specific gravity of all the liquids eligible for examination, then in order that there may be no redundancy of length in the tube E C, we must take $\mathrm{H} = h_1$; in this case equation (4) becomes

$$\frac{d_1}{d} = \sqrt{1 - s_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which expresses the ratio of the diameters of the tubes A B and C D in terms of the specific gravity of the lightest liquid. Moreover we have

$h_1 = \frac{h}{s_1}$, and the descent of the water in CD $= h_1 - h$.

Taking $s_1 = \cdot 75$, we get $\frac{d_1}{d} = \frac{1}{2}$; and taking $h = 10$, we get $h_1 = \frac{10}{\cdot 75} = 13\frac{1}{3}$, and $h_1 - h = 13\frac{1}{3} - 10 = 3\frac{1}{3}$.

Hence the following dimensions of the different parts of the instrument may be adopted: AB $= 13\frac{1}{3}$ inches; DC $= 3\frac{1}{2}$ inches; d or diam. DC $= 1$ inch; and d_1 or diam. AB $= \frac{1}{2}$ inch.

In order to show the advantage derived from the enlargement of the tube CD, take $d = d_1$ in equation (4), then

$$\mathrm{H} - h_1 s_1 = h_1, \quad \therefore \mathrm{H} = h + h_1,$$

which shows that, in order to have the columns h and h_1 the same as in the foregoing case, H or E C must be equal to the sum of these columns, which is h in excess of the length determined by the economic condition expressed by equation (5). For example, when $h_1 = 13\frac{1}{3}$, and $h = 10$, then $\mathrm{H} = 23\frac{1}{3}$.

The advantages of the new areometer, as compared with the areometer in common use, are as follows:—

1. A comparatively small quantity of the liquid is required in order to find its specific gravity. It has been shown that with this instrument about an ounce weight of the liquid is sufficient for determining its specific gravity, whereas with the common areometer it requires not less than seven ounces.

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2. It is quite as delicate in its indications as the very best forms of the common areometer, which should consist of at least two instruments,—one for determining the specific gravity of liquids that are denser than water, and the other for those that are lighter than water.

3. The great advantage of the common areometer consists in the simplicity of its application, no balance being required; the manipulations connected with the use of the instrument here proposed are quite as simple, and at the same time present striking experimental illustrations, suited to the lecture-table, of the relative densities of different liquids.

4. The scale of comparison in the common areometer is empirical, whereas that of the proposed instrument depends directly on a fixed and invariable scientific principle.

With these advantages, the areometer here proposed may fairly take rank with the ordinary areometer as an instrument capable of determining, within certain limits of error, the specific gravity of different liquids.

Hastings, February 28, 1859.

XXXIX. *Notes on Mineralogy.*—No. VIII. *On the Felspar and Mica of the Granite of Canton. By the* Rev. Samuel Haughton, *M.A., F.R.S., Fellow of Trinity College, and Professor of Geology in the University of Dublin**.

THE granite of the neighbourhood of Canton is composed of grey quartz, a light flesh-coloured or creamy-white felspar, in large crystals, and a black glossy mica (crystals $\frac{5}{4}$ by $\frac{1}{2}$ inch) imbedded in the felspar and accompanied by quartz.

The following analyses will show the chemical character of these minerals:—

Felspar of Canton Granite.

	Per cent.	Atoms.	
Silica	64·48	1·433	
Alumina	19·12	0·367	0·374
Peroxide of iron	0·56	0·007	
Lime	0·45	0·016	
Magnesia	trace		0·386
Potash	12·52	0·266	
Soda	3·24	0·104	
Loss by ignition	0·16		
	100·53		

From the preceding analysis may be deduced the following relation among the atoms of silica, peroxides, and protoxides:—

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Silica	1433	4·00
Peroxides	374	1·04
Protoxides	386	1·08

From which it is plain that this felspar is orthoclase.

The analysis of the black mica is as follows:—

Black Mica of Canton Granite.

	Per cent.		
Silica	35·50	...	0·789
Alumina	20·80	0·400	0·646
Peroxide of iron . .	19·70	0·246	
Lime	0·56	0·020	0·699
Magnesia	4·46	0·223	
Protoxide of iron . .	7·74	0·215	
Protoxide of manganese	1·70	0·047	
Potash	9·00	0·191	
Soda	0·10	0·003	
Loss by ignition . .	0·25		
	99·81		

From the preceding we obtain, in atoms,—

	Atoms.	Oxygen ratio.	
Silica	789	789	
Peroxides	646	646	879
Protoxides	699	233	

from which may be deduced the following:—

$$\left\{26(3\,RO)+74\,R^2O^3\right\}+90\,SiO^3,$$

or

$$\left\{\frac{1}{4}(3\,RO)+\frac{3}{4}\,R^2O^3\right\}SiO^{3\frac{9}{10}}.$$

These formulæ represent the analysis, and are sufficiently near to the formulæ for the Lepidomelane of Soltmann, and of the black uniaxal micas of Donegal and Leinster, to render it probable that they all are varieties of the same mineral. To show their analogy and difference, the following comparison may be useful:—

	Atoms of			
	Silica.	Peroxide.	Protoxide.	Water.
Lepidomelane	831	569	551	66
Black mica of Ballyellin*.	790	624	538	477
Black mica of Ballygihen*	804	647	515	433
Black mica of Canton . .	789	646	699	139

* Quart. Journ. Geol. Soc. London, vol. xv. p. 129.

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The mineralogical formulæ of the four minerals are:—

I. *Lepidomelane*:

$$\left[\frac{25}{100}(3\mathrm{RO}) + \frac{75}{100}(\mathrm{R^2O^3})\right]\mathrm{SiO^{3\frac{11}{8}}}.$$

II. *Black Mica of Carlow*:

$$\left[\frac{23}{100}(3\mathrm{RO}) + \frac{77}{100}(\mathrm{R^2O^3})\right]\mathrm{SiO^{3\frac{18}{8}}}.$$

III. *Black Mica of Donegal*:

$$\left[\frac{21}{100}(3\mathrm{RO}) + \frac{79}{100}(\mathrm{R^2O^3})\right]\mathrm{SiO^{3\frac{12}{8}}}.$$

IV. *Black Mica of Canton*:

$$\left[\frac{26}{100}(3\mathrm{RO}) + \frac{74}{100}(\mathrm{R^2O^3})\right]\mathrm{SiO^{3\frac{9}{10}}}.$$

It appears to me that the preceding formulæ, representing black micas from Russia, Ireland, and China, balance around a mean or average formula, which may be regarded as the type species of this mineral; viz.—

$$\left[\frac{25}{100}(3\mathrm{RO}) + \frac{75}{100}(\mathrm{R^2O^3})\right]\mathrm{SiO^3}.$$

This abstract or theoretical black mica, probably exists only as an idea or conception in our minds, and may not have a concrete development in any place; but it must be regarded as an essential constituent of the original granite formed in the astronomical epoch by the cooling of our globe. All our researches tend to prove that there is an original or type-granite, characteristic of the azoic epoch of the earth's history, marked mineralogically by the presence of four important minerals,—

1. Quartz;
2. Orthoclase felspar;
3. Black mica;
4. White mica;

and marked chemically by the abundance of potash and the absence of lime.

Trinity College, Dublin,
March 10, 1859.

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XLI. *On Bromo-arsenious Acid.*

By WILLIAM WALLACE, *Ph.D., F.C.S.**

IN former papers† I have described chloro-arsenious acid and iodo-arsenious acid, compounds which consist of arsenious acid in which an equivalent of oxygen is replaced by chlorine or iodine. In the present communication I purpose to notice another member of the same series of compounds—bromo-arsenious, or arsenious acid with one equivalent of oxygen replaced by bromine. This compound is intermediate in its properties between the corresponding acids containing chlorine and iodine respectively.

Terbromide of arsenic is readily formed by very slowly introducing an excess of powdered metallic arsenic into bromine contained in a tubulated retort. The arsenic takes fire as soon as it comes in contact with the bromine. By distilling twice, the bromide is obtained perfectly pure, as a white, fibrous, crystalline mass. When a considerable quantity of fused bromide of arsenic is cooled very slowly, crystals of more than an inch in length are readily obtained. From these the still liquid portion may be poured off, and thus the bromide is prepared in a state of perfect purity. I intend to employ the bromide purified in this manner for the redetermination of the equivalent number of bromine.

Bromo-arsenious Acid.

Fused terbromide of arsenic readily dissolves a considerable quantity of arsenious acid, forming a slightly viscid, dark-coloured fluid, which does not solidify so readily as the pure bromide. When this liquid is gradually distilled until it becomes rather thick, and allowed to cool to about 150°, it separates into two fluids, the heavier being very viscid. The upper liquid consists of bromo-arsenious acid, while the lower is a compound of that body with arsenious acid. Bromo-arsenious acid, thus formed, is a soft, unctuous, semisolid mass, having a dark colour, which does not appear to be owing to the presence of any impurity. Analysis gave—

Arsenic		1 = 75		43·86
Bromine . . .	47·10	1 = 80		46·78
Oxygen		2 = 16		9·36
		171		100·00

$AsBrO^2$ or $AsBr^3 + 2AsO^3$. The more viscid mass which separates from the above appears to consist of $3AsBrO^2 + AsO^3$, or

* Communicated by the Author.

† Phil. Mag. vol. xvi. p. 358, and vol. xvii. p. 122.

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$As Br^3 + 3 AsO^3$. Analysis of three different preparations gave,—

Arsenic . .	...	...	...	4	= 300	49·02
Bromine .	39·2	40·1	41·87	3	= 240	39·21
Oxygen . .	...	...	...	9	= 72	11·77
					612	100·00

The application of a high temperature causes the decomposition of both of these compounds, with distillation of pure terbromide of arsenic. A portion of the bromine is, however, tenaciously retained by the arsenious acid that remains behind.

Action of Water upon Bromide of Arsenic.

Bromide of arsenic cannot be dissolved in water without causing the separation of a white precipitate. About three parts of water at the boiling temperature are required for complete solution; but a much smaller quantity is sufficient if hydrobromic acid is added. A boiling aqueous solution deposits, on cooling, octahedral crystals of arsenious acid, which are quite free from bromine. When the bromide is boiled with a quantity of water containing hydrobromic acid, insufficient to dissolve the whole of it, the portion that remains undissolved becomes viscid, and acquires a dark-brown colour from its conversion into bromo-arsenious acid.

Hydrated Bromo-arsenious Acid.

A cold solution of bromide of arsenic in water and hydrobromic acid gives, by evaporation over oil of vitriol, thin, white, pearly crystals, which consist of the hydrated compound acid. Analysis gave 40·55 per cent. of bromine, which agrees with the formula $3 HO, As BrO^2$.

Arsenic	1	= 75	37·88
Bromine. . . .	1	= 80	40·40
Oxygen	2	= 16	8·08
Water	3	= 27	13·64
		198	100·00

This compound does not appear to lose its water of hydration, or at least not the whole of it, over oil of vitriol.

When bromide of arsenic is dissolved in boiling water containing a considerable quantity of free hydrobromic acid, the solution on cooling gives no arsenious acid, but a compound of arsenious acid with hydrated bromo-arsenious acid, having exactly the constitution assigned to the corresponding iodine salt. This compound falls as a bulky precipitate consisting of white pearly flakes, which have, when drained and dried by pressure between folds of bibulous paper, a beautiful silky lustre. It contains,

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like the iodine compound, for each equivalent of arsenic three equivalents of water, only half of which, or 9·55 per cent., is removed by exposure over oil of vitriol. On exposure to heat in a water-bath, water is first evolved, then a little hydrobromic acid, and subsequently bromide of arsenic: the whole of the bromine is expelled by continued exposure to the temperature of the water-bath. Analysis gave—

Arsenic	...	4 = 300		52·08
Bromine	13·70	1 = 80		13·90
Oxygen	...	11 = 88		15·27
Water	16·54	12 = 108		18·75
		576		100·00

$As BrO^2$, $3AsO^3 + 12HO$, or $AsBr^3 + 11AsO^3 + 12HO$.

When a concentrated cold solution of bromide of arsenic is treated with bromide of ammonium, well-formed six-sided tables with bevelled edges are slowly formed. These consist of anhydrous bromide of arsenic, with a small proportion of bromide of ammonium mechanically mixed with it. Deducting 9·24 per cent. of bromide of ammonium, analysis gave—

Arsenic . .	...	...	1 = 75		23·81
Bromine . .	76·40	75·75	3 = 240		76·19
			315		100·00

Bromo-arsenious acid does not appear to form compounds with the alkaline bromides.

XLI. *Remarks on the Veined Structure of Glaciers.* By John Ball, *M.R.I.A., F.L.S.**

PROFESSOR TYNDALL having done me the honour to refer, in his recent lecture upon glacier structure at the Royal Institution, to an article which I contributed to this journal in December 1857, and having intimated that it had the fortunate result of inducing him to examine, still more closely than before, some of the phænomena of glaciers, I trust I may be permitted to offer a few remarks upon the present state of the question which has been in debate—the origin of the veined structure in glacier ice.

I must commence by avowing my conviction that, substantially, Professor Tyndall has been victorious over those, myself included, who were at first disposed to doubt his theory, that pressure is the efficient cause to which the veined structure generally, if not universally, owes its origin. This victory has

* Communicated by the Author.

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been gained by the continued study of the subject, which has induced the author of the new theory to modify very considerably the views which he first published two years ago. Having been sagacious enough to perceive the true cause of the phænomenon in question, he has been led to alter his view as to the *modus operandi*: his theory is no longer a merely mechanical one, in which it is assumed that pressure must have upon the particles of glacier ice the same effect that it has upon those of mud; it is now a physical theory, depending upon the known properties of the particular substance of which glaciers are composed, and one capable of being directly brought to the test of experiment.

Having frankly admitted so much, I claim permission to point out one or two particulars in which the chain of demonstration framed with so much knowledge and ingenuity seems to me still incomplete, and to urge that there may be still some residual phænomena in glacier structure not accounted for by Professor Tyndall's theory, and capable of interpretation through the action of other physical causes.

The conclusive argument against the so-called stratification theory is derived from the remarkable appearances which Professor Tyndall observed last summer on the Furgge Glacier near Zermatt. The situation was one which exposed him to considerable danger in approaching close to the ice, and the illustration exhibited at the Royal Institution did not look very like the ordinary veined structure; but in the case of so bold an alpine traveller, and so practised an observer, I have no doubt whatever of the complete accuracy of the statement, that in that place the veined structure was seen cutting through the planes of the stratification of the névé, and, further, that Professor Tyndall satisfied himself that this veined structure is developed in a direction perpendicular to that of pressure acting on the glacier. Further than this, I am quite satisfied with the accuracy of the more general statement, that in the three ordinary cases in which we are able to detect the veined structure, the surfaces that compose it are disposed at right angles to the direction in which pressure is actually at work, or has previously acted, upon the glacier ice. Is it therefore necessary to believe that the veined structure is, in every case, a product of pressure acting on the ice? Upon this point I venture to retain some doubts, which I desire to submit to those who may be disposed to pursue, into their last recesses, the varied problems to which the glaciers have given rise. I turn from the great glaciers arising from the confluence of many different ice-streams, where the veined structure is found in ice many miles distant from the spot where it was deposited hundreds, it may be thousands,

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of years before, to direct attention to those smaller accumulations that are found on slopes and depressions of the Alps, not far below the region of the névé. I do not now speak of those small patches of permanent névé which have scarcely any measurable motion, one of which, close to the summit of the Faulhorn, was very carefully studied by that good observer M. Charles Martins; and of which, as far as I can judge from the lithograph, an example is given in plate 9 of Forbes's 'Travels through the Alps.' I speak of true glaciers of the second order, such as the Kaltwasser Glacier above the pass of the Simplon, the motion of which was measured by Professor Forbes. Examples may be found on all the higher parts of the Alps, but most commonly on mountains of 10,000 or 11,000 feet in height: they possess an appreciable, though slow, onward motion; and the névé becomes completely transformed into glacier ice. Such glaciers are usually of very moderate depth, and are nearly free from crevasses; so that the only favourable opportunity for obtaining a view of their internal structure is in places where they come to an end over an edge of steep rocks, and the successive portions of the glacier break away as they advance over the edge. In every instance of this nature, where I have been able to approach the ice, I have seen indications of a structure quite undistinguishable from the ordinary veined structure, formed in planes sensibly parallel to the bed of the glacier. I would strongly recommend those who desire to approach glaciers, however small they may be, in such situations, to be on the ground early in the morning, before the sun has had time to loosen impending fragments of the glacier. Failing this precaution, considerable risk attends such inquiries.

In the cases to which I refer, it has appeared to me that it is difficult to conceive the action of any pressure adequate to develope the veined structure. The only pressure, indeed, that can be called into play is that compounded of the weight of the upper portion of the ice pressing on the lower, together with that which Professor Tyndall has shown to exist where adjoining portions of a glacier move with unequal velocity. Whether the amount of these two forces combined can be sufficient to generate the veined structure in a glacier but thirty or forty feet deep, where the absolute rate of advance is very small, and the differential motion is still smaller, seems to me very questionable. If it be so, the structure so developed should, at all events, be confined to the lowest beds of such glaciers; but, as far as my observation has gone, this is by no means the case. The structure has appeared uniform throughout the thickness of the glacier. Pending further inquiry on this branch of the subject, I shall venture to believe that in these cases the veined

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structure is, in fact, a form of stratification, arising from the gradual consolidation of the original beds of névé. I have said that it is undistinguishable from the ordinary veined structure which I now believe to be due to pressure; but it is probable that closer examination would show that the structure produced by two different causes is similar only, and by no means identical. It seems probable, for instance, that the layers constituting the structure will be found to be more continuous in glaciers of the second order—if there it be really a result of stratification—than in the great glaciers, where it arises from pressure. The peculiar molecular condition of the ice which causes cleavage may also be a characteristic of the Pressure Structure, as distinguished from the Stratification Structure.

Turning from this (which, at the utmost, would merely establish an exception to the ordinary law regulating the origin of the veined structure) to the fundamental experiment upon which the physical theory of the subject is now based, it will certainly not have escaped the acute mind of its author that experiments upon solid ice, though they may furnish fair ground for inference, fall a good way short of demonstration as to the result of pressure upon that curious mixture of ice, water, and air which constitutes the mass of ordinary glacier ice. Close examination shows it to be a sort of breccia, or conglomerate, made up of angular fragments of ice closely adhering together, filled throughout with air-bubbles, each separate fragment having the small air-cells more or less flattened in the same direction, but no general parallelism in the direction of flattening being traceable amongst adjoining portions of the ice. The cells are sometimes —Mr. Huxley thinks, always—partly occupied by water along with the air which they contain. After seeing the experiments by which Professor Tyndall shows that mere pressure applied to a mass of pure ice, will, without change of temperature, cause partial liquefaction extending in fissures transverse to the direction of pressure, a person, arguing *à priori* as to the effect of intense pressure upon such a mass as glacier ice, would be apt to conclude that liquefaction would proceed on the free surface of the interior of each air-cell, and in such a direction as would enlarge the cells into the form of lenses sensibly parallel to each other. But, instead of this, we find the air-cells in the white veins showing no signs of change, while, at moderately regular intervals, we find veins from which nearly all the air-bubbles have been driven out. I am far from putting this as an objection to the pressure theory; it seems to be an example, but surely a very remarkable one, of an extensive and still obscure class of physical phænomena, wherein force, transmitted through a resisting medium, manifests itself in apparent intermittence,

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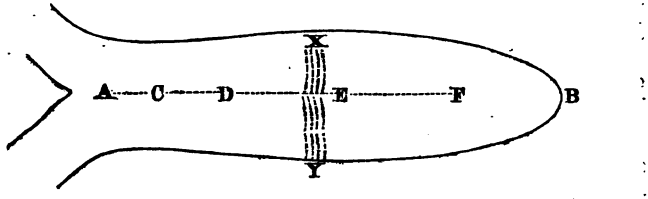
with alternations of maxima and minima. All that I contend for is, that the subject is one important and interesting enough to deserve still further illustration and experiment. With this object, I would suggest that ordinary snow, névé, and glacier ice should each be subjected to powerful compression in chests containing about a cubic yard, of which the sides should be elastic to allow of some lateral expansion. The effects of greater compression, applied for a short time, might be compared with those of a lesser pressure, continued for months, or even a longer time.

It appears probable that a very considerable amount of pressure must be necessary to produce the structure, as otherwise we should more frequently find it disposed horizontally, developed by the pressure of the upper on the lower parts of a glacier.



To make this clear, let A B be a glacier formed by the confluence at A of two smaller glaciers, from the mutual thrust of which the veined structure will be developed in vertical planes parallel to the medial moraine. At C, where the glacier lies in a uniform bed with a moderate slope, that thrust will have ceased to operate; but the veined structure once created remains visible, and is no less visible at D, a point a mile lower down the glacier. But if the average rate of advance be 200 feet in the year, and the average annual ablation, or removal of the surface by melting, be 4 feet—both reasonable estimates—the ice which we see on the surface at D must have been 104 feet deep when it was under the point C. The absence of all trace of horizontal structure in such positions, leads to the inference that the mere pressure of the ice upon its own lower beds is not in general capable of developing the veined structure.

The same diagram will serve to suggest an explanation of a difficulty felt by some observers, and which, I suspect, led even Professor Forbes into a slight error. Let XY represent an ice-cataract, E a point at its base, F a point further down the glacier —say one mile distant from E. At E, for the reason explained by Professor Tyndall, the veined structure is developed across the glacier, parallel to XY; but lower down, at F, it is seen in the old position, parallel to the medial moraine. It is inferred that

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some force must have been at work in the space between E and F to effect this change in the direction of the planes of the veined structure. Nothing of the kind, however, need take place. The compression at E, and the resulting transverse structure, are merely superficial as compared to the entire thickness of the glacier. In the neutral zone, which has undergone neither compression nor tension during the passage of the ice-cataract, the structure originally developed at A may well survive, and when superficial waste has cleared away the portion affected by transverse compression, the old longitudinal structure comes again to the surface.

Why we never see indications of a *cross-bar* structure in cases where pressure is applied in a new direction to ice already possessing the veined structure, is a question that seems to me to deserve further examination. Experiments such as I have already suggested, which should include this inquiry, might probably give interesting results.

Before closing these remarks, I beg to call the attention of observers to a point of glacier structure which has obtained but little notice. At the lower extremity of glaciers, where a stream issues from a cavern in the ice, there is almost always apparent a tendency to split in curved surfaces, which, in part at least, cut perpendicularly the planes of the veined structure. It is to this tendency that the caverns whence the glacier torrents issue owe their arched form. It is clear that this is no merely accidental hollowing out of the cavity above the stream; for above the cavern successive fissures may be seen, sometimes but an inch or two in width, all strictly parallel to the curve of the interior arch. Does this indicate a structure extending throughout the entire of the glacier? I have sometimes thought so, and imagined it to be analogous to the jointing of crystalline rocks; but I do not venture to speak with confidence on the subject. I believe partial subsidences near to the banks, and even in the central region of great glaciers, to be more common than is usually supposed, and to be accompanied by fissures which tend to cut at right angles through the surfaces of the veined structure.

In conclusion, I beg to express the conviction that, with the key to a true explanation of the most important phænomena of glacier structure, for which we are indebted to Professor Tyndall, a systematic inquiry into the application of his theoretical views to all the facts presented in the ice-world, and into those residual phænomena to which he has not turned his attention, would amply reward the labours of a competent observer, and is required, in spite of all that has been hitherto done, to complete our knowledge of the glaciers.

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XLII. *On the Stratification of Electric Light.* *By the* Rev. T. R. Robinson, *F.R.S. &c.**

In a communication on the Stratification of Electric Light in rarefied Media, which appeared in the Philosophical Magazine for last July, Mr. Grove has described some facts which are in close relation to the cause of that phænomenon, of which the most important is this—If in the circuit of an induction-machine of which an exhausted vessel forms a part, there be an interruption which is gradually lessened till sparks just pass it occasionally, those sparks are blue, and have a single sharp sound; if the interval be still more diminished, they become yellow, burred, and their sound is not so clear, but is attended with a slight whiz. Now he finds that the blue sparks do not form strata in the vacuum, but that the yellow do, so that by regulating the distance he can produce them or not at pleasure. He *thinks* the blue are single, and the yellow double or multiple; and finds in this a proof of his former opinion, that these strata are caused by some peculiar action of compound discharges. Within the last few weeks he has developed these views more fully in a lecture at the Royal Institution.

Everything which comes from Mr. Grove bears the stamp of sagacity and power; and I read this paper with great interest; but circumstances prevented me till recently from repeating its experiments. The results which I have obtained, show that its leading inference cannot be received as universal; for, *in the circumstances of my experiments*, I always obtained strata when a spark passed, whether long or short, blue or yellow. Any one versed in these inquiries knows how much the strata are modified by slight variations of apparatus, &c.; and some such have probably caused this discrepancy; I will therefore describe mine in some detail.

The induction-machine which was mostly employed, consists of six sectional coils (the gift of my friend Dr. Callan), each containing 8000 feet of fine iron wire, not lapped, but insulated by an elastic varnish. They are arranged, three on each of two vertical primaries, having each 350 spires of No. 12 copper, and cores of iron No. 17. Their internal terminals are below, connected in the centre; and the current is passed collaterally through the primaries, so that the external terminals of the extreme coils have equal and opposite tensions. The condenser has 100 square feet of charged surface, which can be used in sections of 20 according to the battery power. The rheotome is mercurial†; and the machine gives sparks of about one inch for every Grove's cell used to excite it. A few words respecting

* Communicated by the Author.

† This kind of rheotome is the best which I have tried, giving sparks

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these sparks may be found relevant to the present question. If the terminals be connected with a spark-micrometer opened to about twice the striking distance, when the machine acts, a lucid star will appear on one or both points. Bringing them gradually closer, a small brush, exactly like that produced by a point on a prime conductor, is seen at the positive point: as the distance is lessened, the filaments of this brush extend, and at last curve towards the negative with a sputtering sound. Still nearer, and sparks strike across with an intense light and sharp snap which cannot possibly be mistaken for the preceding form of discharge: they are zigzag; and, when the excitation is powerful, two-thirds of their length at the positive end is often red, the rest bluish white. If the distance be less, the spark has that strange yellow envelope which, as Du Moncel has shown, can be blown aside like a flame, but which is certainly not a second discharge*. And at very short distances there is sometimes a sheaf of curved sparks between the points. If one terminal be connected with the gas-pipes of the house, and the arm of the micrometer which had been joined to it be connected with the floor (so that the circuit includes a very great resistance), we obtain what is called the static discharge, which is of the same character as the inductive one discovered by Mr. Gassiot, and, like it, can be distinguished from the dynamic one by the magnet when passing through a vacuum and the revolving mirror in air: it is about half the length of the other. The vacuum which I included in the circuit was an "electric egg," 9′ high and 6′ diameter; on its wires were cemented glass tubes with Wollaston's points of platinum, $\frac{1}{60}$ diameter, 6′ apart. This form of electrodes reduces the conditions of discharge to a more definite state than when they are balls or naked wires; and the following may be considered its normal character when the egg has been filled with dry hydrogen and exhausted to 0′·08. Supposing the upper electrode positive, there is at it a brilliant

whose length is 1·3 times that of those obtained by the spring rheotomes of Hearder or Ladd: Ruhmkorff's vibrating hammer is much feebler. This very energy, however, tests the insulation of the coils severely. I find two precautions necessary in using it:—(1) the mercury must be negative, otherwise the action is almost explosive, and the effect only $\frac{1}{3}$; (2) the platinum points should work through a diaphragm of thin vulcanized rubber, to prevent the blackened alcohol from being splashed about.

* When such a spark is viewed in a revolving mirror, its thickness is slightly increased, but the envelope is drawn out into a sheet extending many degrees, even when the rotation is comparatively slow, which shows that it lasts much longer than the spark itself. This explains a singular fact which has occurred to me occasionally. In a hydrogen vacuum =2′·50, if a small Leyden jar be connected with the machine, according to Mr. Grove's plan, the discharge passes as a splendid scarlet spark; this is sometimes surrounded by a faint elliptic envelope, which continues visible for 0″·1 after the other has disappeared.

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point, from which streams an elliptic spindle of greenish light for about two-thirds of the distance, brightest in the axis: this is full of strata, thick near the point and curved round it like spheric shells, thinner below and less curved, till at the lower termination of the positive light they are nearly plane and only 0′·05 or 0′·06 thick. In the bright central portion they seem thicker than their continuations into the surrounding part, which are also bent back abruptly. If the excitation be very powerful, the curvature of the lower strata is reversed, these being in that case formed, as Mr. Gassiot has shown, by that current which passes on closing the primary circuit. Below the positive light is a dark space from 0′·5 to 2′ wide; and lower still is an atmosphere of bright blue light (which I call the aigrette). It is generally conical with a convex base, *never shows strata*, but seems to be composed of rays diverging from the negative point. From this, however, it is separated by a thin dark space surrounding it like a wrapper, and within that by a reddish-violet one. Along the glass tube, below this, positive light with its strata reappears; due probably to the difficulty of insulating the wire where the tube ends. If static discharges be passed, the appearances *at each point* are similar—a negative aigrette, a dark space, and a few thick spherical strata at each end of the faint spindle of light concave towards the nearest point. This is explained by the double nature of this discharge. If a static spark be viewed in the mirror revolving eighty times in the second, it is seen to consist of two, the second narrower than the first and less luminous, about 5° behind it, that is, nearly $\frac{1}{6000}$th of a second later: the two being opposite in direction, produce two reverse systems, which are merely superposed.

Having premised so much, my trial of Mr. Grove's experiment can be easily described. Exciting the induction-machine by two Grove's, it gave static sparks = 0′·90, and dynamic = 2′·227 at the rate of 7 in 2ˢ. With the egg in circuit, no sparks passed till the micrometer was at 1′·038. During the whole of the previous star- and brush-discharges, the appearances in the egg were those which I have described as static, strata and aigrettes *at each* end, even when the micrometer was at 4′. The moment a spark passed (and, as I have already said, these cannot be mistaken), the strata appeared in their normal condition, merely increasing in brightness and magnitude till the micrometer was in contact. By no manipulation of it or the rheotome could I get a spark without producing them; nor, indeed, could I get any discharge through the vacuum without getting at least the static set.

On another occasion with three Grove's, the static spark was

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1′·202 and the other 2′·925; but, as before, I could get no *discharge without strata*. Then I examined the sparks, which were actually showing them splendidly, in the revolving mirror at the highest speed which I could manage, 120 in a second. There may have been a few multiple, but only as exceptional; for in almost every instance they were certainly single with that speed, which would easily show $\frac{1}{3000}$ of the second. I substituted for the egg one of Mr. Gassiot's magnificent Torricellian tubes (for which, as well as much precious information and personal kindness, I have to thank that gentleman). It is 1′ diameter, and 31′·3 between the platinum points: whenever a spark passed the micrometer, its peculiar strata appeared in their normal beauty; but with the brush, they had the peculiar character which Mr. Gassiot has shown to belong to discharges made by induction through the glass; and placing the tube axial on a powerful electro-magnet, it showed, as in his trials, the brush discharges to be double in opposite directions, or, as he calls them, "reciprocating."

These are not the only trials which I have made; but I invariably found that no length of spark prevents the formation of strata, and I am obliged to conclude that Mr. Grove's rule is not absolute. The difference between our results depends doubtless on some unnoticed difference in the conditions under which we operated. Among possible causes may be named (1) the nature of the vacuum. He used air with phosphorus vapour diffused in it; I hydrogen: I prefer that, as definite in nature, as with equal air-pumps giving a rarefaction fourteen times greater, and as exhibiting these strata better than any medium with which I am acquainted, except mercurial vapour. (2) The sort of electrodes. Believing the strata to depend on a peculiar mode of disruptive discharge, I think they will be formed most certainly when the electric power is concentrated on a narrow area, as in the guarded points which I used. (3) The direction of the current. When the upper electrode is positive, they are better developed than when it is negative; in the latter case there are often only a few large ones near the lower point, and the rest are lost in luminous haze, the ascending currents of the heated medium probably confusing them. (4) Still more important is the intensity of the electricity: with very weak power (which would give an air-spark of 0′·1 or 0′·2) I have failed in obtaining them, even in the Gassiot tube. On the other hand, excessive power fails also, but in a different way, producing but concealing them. If, when my six coils are highly charged, the discharge of one, two, &c. be passed in succession, it is seen that the bright strata throw out cloudy appendages into the dark intervals as the intensity increases, so

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that with one giving 5′ sparks, the latter are filled with light. Mr. Gassiot found the same effect from increasing the number of battery cells; and I believe that his gigantic American machine scarcely shows any stratification.

But even were it universally true that a spark of sufficient length interposed in the circuit prevents the appearance of strata, still Mr. Grove's theory of their origin would remain subject to weighty objections. We have no experimental evidence that the current which he supposes to succeed the extra current in the primary coil, exists with any sensible energy; and, granting its existence, it is not easy to see how it can produce the effects assigned to it; for, apparently, it must be *subsequent* to the discharge of the secondary coil, and therefore cannot modify that in any way. A synchronous one, we know from experiment, only weakens the force of another that is opposite; and in the static discharge, where there is the very system which he requires, a discharge followed at a very small interval by a weaker opposite one, there is certainly no special power of developing strata.

A different view of their origin, and one which seems nearer the truth, is given in the Number of 'Cosmos' for the 4th of last month, by M. Morren of Marseilles. He thinks they are caused by periodical variations of intensity in the current, due to the resistance which it meets in traversing an imperfect conductor, and that these cause lateral discharges of the conducting material; he therefore compares them with the wings that project from the stains made by exploding fine wires over paper by an electric battery. The notice is so brief, that I supposed he meant to represent these explosion-pictures as "autographies des stratifications de la lumière électrique;" but the meaning of this phrase is made clear by another notice in the same journal (Feb. 18) from M. Seguin, who also seems to have obtained the same result. An induction spark sent along glass dusted with fine charcoal, leaves a track whose markings he considers identical with the strata. Undoubtedly these variations of intensity do exist: they are shown by the fracture of a wire into minute pieces when the discharge is not quite sufficient to fuse it, and still more plainly by sending the static discharge of a powerful induction machine through a fine steel wire some feet long. In air of ordinary density, and still more in rarefied air, the wire is luminous; but at every inch or two it throws out a circle of brushes. In the exploded wire, or the air over the glass, the same thing happens; but the brushes carry with them most of the metallic vapour or the charcoal dust, leaving a deficiency of them at the intermediate points. On repeating M. Seguin's experiment, I obtained the appearances which he describes:

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they have a strong resemblance to the explosion-pictures, and also to the yellow envelope already referred to as surrounding short sparks; but, as it seems to me, their analogy to the strata is far from complete. The transverse divisions scarcely ever go entirely across, and have no regularity; the jagged fringes and serrated points which form the outline, are in strong contrast with the smooth and comparatively definite boundary which the light often shows; but, above all, the markings extend through *the whole* track of the discharge, and there is nothing analogous to the dark space or the blue negative light. In fact the two sets of phænomena seem to belong to different categories: one is the transfer of matter laterally from the axis of discharge by a vehement repulsion; the other is a succession, along a certain portion of that axis, of fits of discontinuity in the light-producing power of the current. That power, for a certain further distance, totally ceases, to reappear without any intermission, and with the development of rays of higher refrangibility. It is certainly possible that, in rarefied air, these so-called autographs might assume a similar character; but unless this prove to be the case, I think it will be felt that some further step is necessary to complete the explanation. As it now stands, any one who compares a fine set of strata with (for example) the superb drawings of exploded wire in Van Marum's *Description d'une très-grande machine Electrique*, will scarcely admit them to be results of the same action, that of mere repulsive force.

It has been for some time my own opinion that the strata are caused by these periods of intensity, but in a different way from that just mentioned,—by the successive zones along the axis becoming charged up to the point of disruption. I feel, however, that any exposition of it must be premature till more facts are collected, and still more till we have a mathematical investigation of a current's motion in an imperfect conductor. While such labourers as Faraday, Gassiot, and Grove himself are in the field, we can have little doubt that the harvest will not be long unreaped; and we may surely expect that some powerful mind will ere long bring within the domain of analysis the hypothesis (which every day confirms) that electricity is, as Grove expresses it, "a mode of motion." Such an investigation is, from its correlation to other molecular forces, of the highest importance, and will certainly reward most amply its author.

Armagh Observatory, March 7.

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XLIII. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D., Teacher of Physical Science in the Cheltenham College.*

[Continued from vol. xvi. p. 524.]

WURTZ had expressed the opinion that acetal is glycol, $\left.\begin{matrix}C^4H^4\\H^2\end{matrix}\right\}O^4$, in which two atoms of hydrogen are replaced by two atoms of ethyle. Some experiments he has lately made* have shown that, when the two atoms of hydrogen are replaced by ethyle, the product is not acetal, but a body isomeric with it.

When glycol is treated with sodium, a very energetic action ensues, with liberation of hydrogen and formation of sodium-glycol, $\left.\begin{matrix}C^4H^4\\Na\\H\end{matrix}\right\}O^4$, a white crystalline solid. When this substance is heated with excess of sodium at a high temperature, the compound $\left.\begin{matrix}C^4H^4\\Na^2\end{matrix}\right\}O^4$ is formed, or glycol in which two atoms of hydrogen are replaced by sodium. When the former compound is treated with iodide of ethyle, ethyle-glycol, $\left.\begin{matrix}C^4H^4\\C^4H^5\\H\end{matrix}\right\}O^4$, an etherial body of an agreeable odour, is produced. It is glycol in which one equivalent of hydrogen is replaced by ethyle, and is hence quite analogous to the monoacetate of glycol, $\left.\begin{matrix}C^4H^4\\C^4H^3O^2\\H\end{matrix}\right\}O^4$, or glycol in which an equivalent of hydrogen is replaced by othyle.

When ethyle-glycol is acted on by potassium, hydrogen is liberated, and the compound $\left.\begin{matrix}C^4H^4\\C^4H^5\\K\end{matrix}\right\}O^4$ is produced; and this compound, treated with iodide of ethyle, gives iodide of potassium and diethyle-glycol, $\left.\begin{matrix}C^4H^4\\(C^4H^5)^2\end{matrix}\right\}O^4$.

Diethyle-glycol is a mobile liquid, with an agreeable penetrating etherial odour. It boils at 123°·5; is lighter than water, and insoluble therein. It has the same composition and vapour-density as acetal, but it is only isomeric with it; their boiling-points differ by 20° C.

In glycol, two equivalents of hydrogen replaced by two equivalents of the monoatomic radical C^4H^5, give rise to diethyle-glycol; and it might be supposed that, if the two equivalents of

* *Comptes Rendus*, August 1858.

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hydrogen were replaced by one equivalent of the biatomic ethylene C^4H^4, the ether of glycol, $\left.\begin{matrix}C^4H^4\\C^4H^4\end{matrix}\right\}O^4$, would be formed. This would stand in the same relation to glycol, $\left.\begin{matrix}C^4H^4\\H^2\end{matrix}\right\}O^4$, as does ordinary ether, $\left.\begin{matrix}C^4H^5\\C^4H^5\end{matrix}\right\}O^2$, to ordinary alcohol, $\left.\begin{matrix}C^4H^5\\H\end{matrix}\right\}O^2$. The action might take place in accordance with the equation

$$\underset{\text{Disodium-glycol.}}{\left.\begin{matrix}C^4H^4\\Na^2\end{matrix}\right\}O^4} + \underset{\text{Bromide of ethylene.}}{C^4H^4Br^2} = \underset{\text{Bromide of sodium.}}{2NaBr} + \underset{\text{New body.}}{\left.\begin{matrix}C^4H^4\\C^4H^4\end{matrix}\right\}O^4}.$$

But a recent experiment* made by Wurtz has shown that the action of bromide of ethylene on sodium-glycol is not in accordance with this equation. The substances produced are glycol and bromide of acetyle, C^4H^3Br.

When glycol is treated in the oil-bath at 250° with thrice its weight of recently-fused chloride of zinc, a brisk action is set up, abundant vapours are disengaged, which condense to a liquid, forming three layers,—the lower one of which consists of hydrocarbons, a middle and aqueous one of aldehyde, and an upper one of a volatile substance, the nature of which has not been determined. That the middle layer contained aldehyde was proved by preparing from it aldehyde-ammonia. Propylic glycol treated in the same manner, yields propylic aldehyde. The chloride of zinc acts simply as a dehydrating agent,

$$\underset{\text{Glycol.}}{C^4H^6O^4} = \underset{\text{Aldehyde.}}{C^4H^4O^2} + 2HO;$$

and if the ethers are simply dehydrated alcohols, then the aldehydes are the true ethers of glycol.

Wurtz and Frapolli† have succeeded in transforming aldehyde into acetal. According to the equation

$$C^4H^4Cl^2 + 2(C^4H^5NaO^2) = 2NaCl + C^4H^4(C^4H^5)^2O^4,$$

it might be expected that the chloride of aldehydene, $C^4H^4Cl^2$, the isomer of chloride of ethylene obtained by Geuther by the action of pentachloride of phosphorus on aldehyde, and which had previously been obtained by Wurtz by the same method, by acting on sodium-alcohol, would yield acetal. But Wurtz and Frapolli have found that, although these substances act energetically on each other, the action is not in accordance with the above equation—no acetal is formed, the principal product being a gas, C^4H^3Cl, which was found to be identical in all its pro-

* *Répertoire de Chimie*, November 1858.
† *Comptes Rendus*, September 1858.

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perties with that produced from chloride of ethylene. By the following experiment, however, acetal is formed from aldehyde. Aldehyde is mixed with twice its volume of absolute alcohol and saturated with hydrochloric acid gas; two layers are formed—a lower aqueous layer, and an upper one consisting of a compound intermediate between acetal and chloride of aldehydene, its composition being $C^8 H^9 ClO^2 = \left.\begin{matrix} C^4 H^4 \\ C^4 H^5 \\ Cl \end{matrix}\right\} O^2$, or acetal, $\left.\begin{matrix} C^4 H^5 \\ C^4 H^4 \\ C^4 H^5 \end{matrix}\right\} O^2$, in which chlorine occupies the place of the group $C^4 H^5 O^2$. It is produced in accordance with the equation

$$C^4 H^4 O^2 + C^4 H^6 O^2 + HCl = C^8 H^9 ClO^2 + 2HO.$$

Aldehyde. Alcohol. New body.

When this substance acts upon ethylate of soda, it gives rise to chloride of sodium and acetal,

$$C^8 H^9 ClO^2 + C^4 H^5 NaO^2 = NaCl + C^{12} H^{14} O^4.$$

Oxychloride of aldehydene. Sodium-alcohol. Acetal.

The acetal obtained was found to have all the properties attributed to it by Stas, its discoverer.

Deville and Caron* have described a method by which they have prepared several crystallized minerals. The method employed consists in the action of metallic fluorides on oxygen compounds, either fixed or volatile. It requires a high temperature, but is of very wide application, as the metallic fluorides are seldom absolutely fixed. *Corundum* is prepared easily, and in large crystals, by introducing into a carbon crucible fluoride of aluminium, above which is fixed a small cupel of carbon filled with boracic acid. The whole is fitted with a good cover, and kept for an hour at a white heat. The vapour of the fluoride of aluminium meets that of the boracic acid; and their mutual action gives rise to fluoride of boron and to corundum, which is thus frequently obtained in crystals a centimetre in length, but of no great thickness, having the hardness and all the other physical properties of the natural corundum.

Rubies are similarly obtained, a little fluoride of chromium being added to the fluoride of aluminium; the operation is conducted in crucibles of alumina, and the boracic acid placed in a cupel of platinum. *Blue sapphire* and *green corundum* are produced under similar circumstances, the difference in colour arising from the difference in the quantity of chrome. *Cymophane*, obtained by the action of boracic acid on fluoride of aluminium and glucinum, resembled the American specimens.

* *Comptes Rendus*, April 1858.

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Gahnite was obtained by placing a mixture of fluorids of aluminium and fluoride of zinc in vessels of iron, containing boracic acid placed in a platinum tray. The Gahnite is deposited on the various parts of the apparatus, in very brilliant regular octahedra coloured by the iron.

When the boracic acid is replaced by silicic acid, and volatile fluorides employed, crystallized silicates may be obtained. In this manner staurotide was obtained in form and in composition like the natural mineral. It is also obtained by heating alumina in a current of gaseous fluoride of silicon. The alumina becomes changed into cruciform crystals which have the composition of staurotide.

Rutile was obtained by the decomposition of a fusible titanate, more especially titanate of protoxide of tin, with silica.

According to Lies-Bodart, the metal calcium is readily obtained by the following method:—Equal equivalents of sodium and of iodide of calcium are placed in a wrought-iron crucible provided with a screw lid. The crucible is then fastened down and heated gradually for an hour at a red heat, care being taken not to pass or exceed this. When the crucible is cold, the metal is found on the surface. In one operation, 3 grammes of calcium were obtained with 4 grammes of sodium.

The metal is dull in appearance, which arises from its being covered by a thin layer of a blackish substance, believed by the author to be a suboxide of calcium, and which is readily detached: a surface freshly cut has a pale yellow colour with a reddish reflexion. Calcium requires a red heat to burn in the air; once inflamed, it burns with great brilliancy, projecting sparks. It decomposes water at the ordinary temperature.

Dumas* has confirmed this, but has found that the result is only obtained when the action takes place in closed vessels. By heating sodium with iodide of calcium at the ordinary pressure, the first burns, and the latter remains unchanged.

Brewster, more than thirty years ago, described the occurrence of cavities and liquids in the crystals of various minerals. The cavities in some cases were empty, in others contained air and one or more liquids. They were mostly found in topaz, quartz, and amethyst, but were met with in very many minerals, and among others in diamonds. Frequently in one and the same cavity of a quartz or topaz two liquids were found, one of which was heavier than the other, and but little expansible; the other was lighter, and extremely expansible. The first was doubtless water, it had the refractive index of that substance. On opening the

* *Comptes Rendus*, vol. xlviii. p. 575.

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cavities, the liquid exhibited various comportments; sometimes they vanished instantaneously, and without leaving a trace; at others they disappeared more slowly, leaving a fixed residue. Hitherto no satisfactory explanation has been afforded of these phænomena.

Simmler, in a paper in Poggendorff's *Annalen**, has put forth the view that the expansible liquid may in many of the cases have been liquid carbonic acid.

Brewster first determined the coefficient of expansion of the more expansible liquid, and found that between 10° and 26° it expanded from 1·000 to 1·250, or a quarter of its bulk; between the same limits water would only expand from 1·000 to 1·003; hence the liquid expands eighty-three times as much as water. According to Thilorier's experiments, the expansion of liquid carbonic acid between 0° and 30° C. is from 100 to 145, which gives 0·015 as the coefficient of expansion for 1° C. Now this agrees almost exactly with the coefficient of expansion of Brewster's expansible liquid, which is, between the observed limits, 1·01497 for 1 degree Centigrade.

With reference to the refrangibility of the liquid, Brewster found it to be less than that of water, which is 1·3358, though not alike in all cases. In a Siberian amethyst he found it to be = 1·1106, and in a Brazilian topaz = 1·1311. The refrangibility of liquid carbonic acid does not appear to have been numerically determined. Davy and Faraday say that it refracts light more feebly than water, while Niemann states that its refrangibility is almost equal to that of water.

Thilorier and Mitchell state that liquid carbonic acid is not miscible with water, and floats on it like ether. Brewster observed the same of the expansible liquid.

The expansible liquid was in many cases under great pressure, producing in some cases an explosion when the mineral was broken. Some of the liquids, when the cavities had been bored into, rose slowly to the surface, spread out, and assumed a rotatory motion. Simmler suggests that this phænomenon is explicable on the assumption of a spheroidal condition. Natterer observed that liquid protoxide of nitrogen might be preserved for some time in an open glass. Of compressible gases which might possibly exist in nature, there are, besides carbonic acid, sulphuretted hydrogen, phosphuretted hydrogen, hydrochloric acid, ammonia, and sulphurous acid; and each of these would have been detected by the smell.

In another paper† Simmler suggests that diamond might possibly be also a product of crystallization from liquid carbonic

* December 1858. † Ibid.

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acid. Diamond often contains cavities, and, as Brewster has observed, with accompanying circumstances which point to a strong pressure in the interior, although he does not state whether they contained water.

Brewster explained his observations of the coloured rings with the black cross, around the cavities, by ascribing to the diamond a gummy consistence and vegetable origin. Simmler suggests that it may rather be compared to that of unequally compressed glass.

To confirm this view of the formation of diamonds, it would be necessary to prove that liquid carbonic acid possessed a solvent power for carbon similar to that which bisulphide of carbon has for sulphur, or liquid sulphide of phosphorus for phosphorus. Experiments which Simmler made in this direction with a view of preparing liquid carbonic acid by Faraday's method, gave no results, as the tubes always exploded.

XLIV. *On the Action of Pentachloride of Phosphorus on Malic Acid.* *By* William H. Perkin, *F.C.S., and* B. F. Duppa, *Esq.**

THE study of the action of bromine on acetic acid, with which we have been engaged for some time, has yielded two new acids, representing acetic acid in which one and two equivalents of hydrogen are replaced by bromine.

These acids, bromo- and bibromo-acetic, when treated with hydrate of silver or potassium, decompose, yielding a bromide and two other acids; thus,—

$$\underbrace{C^4(H^3Br)O^4}_{\text{Bromacetic acid.}} + AgHO^2 = \underbrace{C^4H^4O^6}_{\text{Glycolic acid.}} + AgBr.$$

$$\underbrace{C^4(H^2Br^2)O^4}_{\text{Bibromacetic acid.}} + 2AgHO^2 = \underbrace{C^4H^4O^8}_{\text{Glyoxylic acid?}} + 2AgBr.$$

These acids, it will be observed, differ in composition from acetic acid by containing two and four equivalents of oxygen more than it, and, strange to say, although representing two and three molecules of water, appear to be (under ordinary circumstances) *monobasic*.

Now there are two bibasic acids known, bearing a similar relation in composition to succinic acid as the above do to acetic, as may be seen by the following Table:—

$C^4H^4O^4$	Acetic acid.	$C^8H^6O^8$	Succinic acid.
$C^4H^4O^6$	Glycolic acid.	$C^8H^6O^{10}$	Malic acid.
$C^4H^4O^8$	Glyoxylic acid.	$C^8H^6O^{12}$	Tartaric acid.

* Communicated by the Authors.

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From considerations which we shall not discuss here, it appeared probable that malic and tartaric acids might be produced from succinic acid by a process similar to that by which we obtained glycolic and glyoxylic acids from acetic acid, namely, by treating bromine or chlorine replacement acids derived from succinic acid with the hydrate of silver or potassium. Thus with monobromo-succinic acid we should expect the following reaction:—

$$\left.\begin{matrix} C^8(H^3Br)O^4 \\ H^2 \end{matrix}\right\} O^4 + \left.\begin{matrix} Ag \\ H \end{matrix}\right\} O^2 = \underbrace{\left.\begin{matrix} C^8H^3O^4 \\ H^3 \end{matrix}\right\} O^6}_{\text{Malic acid.}} + AgBr.$$

And with bibromo-succinic acid the following:—

$$\left.\begin{matrix} C^8(H^2Br^2)O^4 \\ H^2 \end{matrix}\right\} O^4 + 2\left.\begin{matrix} Ag \\ H \end{matrix}\right\} O^2 = \underbrace{\left.\begin{matrix} C^8H^2O^4 \\ H^4 \end{matrix}\right\} O^8}_{\text{Tartaric acid.}} + 2AgBr.$$

We are now endeavouring to obtain the bromo- or chloro-succinic acids, so that we may fully investigate this matter.

The late experiments of Wurtz on lactic acid show that when it is distilled with pentachloride of phosphorus, it yields a substance, $C^6H^4O^2Cl^2$, which has since been shown to be the chloride of chloropropionyle; for by the action of water it is converted into chloropropionic acid, which, when acted upon by nascent hydrogen, is converted into propionic acid.

We may here mention that we have made similar experiments with glycolic acid. This substance, when distilled with pentachloride of phosphorus, yielded oxychloride of phosphorus and chloride of chloracetyle, which with water decomposed into hydrochloric and *chloracetic* acids; and this chloracetic acid was converted into acetic by the action of nascent hydrogen.

With these results before us, it appeared probable, if malic acid stood to succinic acid as lactic and glycolic stand to propionic and acetic, that by treatment with pentachloride of phosphorus it would yield a chloride which would represent the chloride of chlorosuccinyle, $C^8(H^3Cl)O^4, Cl^2$.

We have made this experiment, and obtained the following results:—

One part of malate of calcium was well mixed with four of pentachloride of phosphorus; this was then placed in a retort connected with a receiver and suitable apparatus for absorbing hydrochloric acid. On applying heat to the retort for a few moments, chemical action set in and continued for some time; after it had ceased, heat was again applied until no more liquid distilled over.

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The distillate was then transferred to a retort and heated. Large quantities of liquid came over at 110° C., which consisted of oxychloride of phosphorus: after a time the thermometer began gradually to rise; when it had reached 160°, the retort and contents were allowed to cool to 120°; dry air was then passed, in order to separate as much oxychloride of phosphorus as possible; heat was again applied, and the product distilled. It commenced distilling at about 170°, the thermometer gradually rising; in fact no fixed boiling-point could be obtained, as the product decomposed with evolution of hydrochloric acid.

The substance thus obtained is a colourless mobile liquid, heavier than water; when added to cold water, it sinks as an oil.

With alcohol it reacts powerfully, producing an etherial body; with ammonia it forms a white, nearly insoluble substance.

We have not been able to obtain this body sufficiently pure for analysis, consequently have had to content ourselves with the analysis of its derivatives.

Action of Water on this Chloride.

This body, when exposed to atmospheric moisture for a few days, is entirely converted into a white solid mass. A similar result is obtained if it be heated with water. Hydrochloric acid is formed, and, on cooling, the liquid deposits the new body; this, when washed with cold water and recrystallized from boiling water, is obtained in a state of purity. Two combustions of this substance gave the following numbers:—

I. ·3715 of substance gave ·5597 of carbonic acid and ·1233 of water.

II. ·4200 substance gave ·6372 of carbonic acid and ·1365 of water.

Per-centage composition:—

	I.	II.
Carbon . . .	41·07	41·2
Hydrogen. . .	3·60	3·6

These numbers agree with the formula $C^8 H^4 O^8$, as may be seen from the following Table:—

		Theory.		Mean of experiment.
C^8	=	48	41·3	41·13
H^4	=	4	3·4	3·60
O^8	=	64	55·3	
		116	100·0	

This acid is evidently fumaric acid; it cannot be maleic, as it is difficultly soluble in water.

The ether obtained by the action of the above chloride on

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alcohol was left in contact with aqueous ammonia; this in the course of a few hours was perfectly converted into a white, nearly insoluble powder. This was well washed on a filter with water and then with alcohol: a portion burnt with oxide of copper gave the following numbers:—

·2590 of substance gave ·4055 of carbonic acid and ·1295 of water,

equal to 42·7 per cent. of carbon and 5·5 of hydrogen. The formula $C^8 H^6 N^2 O^4$ requires 42·1 of carbon and 5·26 of hydrogen. This substance is therefore fumaramide.

From this it evidently appears that, when pentachloride of phosphorus and malic acid are heated together, oxychloride of phosphorus and chloride of fumaryle are formed; thus,—

$$\underbrace{\left.\begin{matrix} C^8 H^3 O^{4//} \\ H^3 \end{matrix}\right\} O^6}_{\text{Malic acid.}} + PCl^5 = \underbrace{\left.\begin{matrix} C^8 H^2 O^{4//} \\ H^2 \end{matrix}\right\} O^4}_{\text{Fumaric acid.}} + PCl^3 O^2 + 2HCl,$$

and then

$$\left.\begin{matrix} C^8 H^2 O^{4//} \\ H^2 \end{matrix}\right\} O^4 + 2PCl^5 = \underbrace{C^8 H^2 O^{4//}, Cl^2}_{\text{Chloride of fumaryle.}} + 2PCl^3 O^2 + 2HCl.$$

Fumaric acid, chloride of fumaryle, and fumaramide bear a very close relationship to succinic acid, chloride of succinyle, and succinamide, as will be seen from the following Table:—

Fumaric acid	$C^8 H^4 O^8$.	Succinic acid	$C^8 H^6 O^8$.
Chloride of fumaryle.	$C^8 H^2 O^4, Cl^2$.	Chloride of succinyle.	$C^8 H^4 O^4, Cl^2$.
Fumaramide	$C^8 H^6 N^2 O^4$.	Succinamide	$C^8 H^8 N^2 O^4$.

the only difference being that the derivatives of fumaryle contain 2 equivs. of hydrogen less than those of succinyle. In fact we think that fumaric acid may be considered as the member of a series of acids running parallel to the ordinary oxalic series of which succinic acid is a member.

We are now engaged with the investigation of the action of pentachloride of phosphorus on tartaric acid.

XLV. *On a Method of finding the impossible Roots of an Equation, in reply to the* Astronomer Royal. *By* Professor Challis*.

In the course of the proof which I gave, in the Philosophical Magazine for February, of the theorem that every equation has as many roots as dimensions, I argued, from antecedent alge-

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braic principles, that it is allowable to put the general algebraic expression $z+y\sqrt{-1}$ for the unknown quantity of an equation, z and y being possible quantities, positive or negative. When this is done, the equation is resolved into two others, containing the two quantities z and y^2, from which, by elimination, a resulting equation containing only y^2 may be obtained. Hence, excepting the case in which y is zero, I inferred that the resulting equation must give a positive possible value of y^2. At this point of the reasoning Mr. Airy submits "that at present we are not entitled to assume that we can obtain a possible value of y^2 from the resulting equation;" and he goes on to argue that a positive possible value of y^2 is proved to be obtainable only in case the last term of the equation is shown to be negative. But the general antecedent argument proved that the equation must be satisfied by a positive possible value of y^2, without reference to the sign of the last term. Of course I am aware, from the way in which Mr. Airy puts his objection, that he does not admit the general argument; but as he has adduced nothing against it, I still maintain it to be good, and will add a few more considerations in support of it.

In arithmetic, the differences and the ratios of quantities are visibly expressed by numbers, and by means of numbers the ordinary rules of calculation, as addition, subtraction, multiplication, &c., are established. In algebra these rules are adopted, not proved; but whereas the letters a and b do not indicate which of the quantities they represent is the greater, it is necessary to operate at the same time *by rules which shall make the operations independent of relative magnitude.* This is effected by the use of the signs $+$ and $-$; and it is the use of these signs for this purpose that constitutes the difference between algebra and arithmetic. The rules of signs, and the various simple forms of functions strictly algebraic, such as $-x$, a^{-y}, $\pm\sqrt{-b}$, $a\pm\sqrt{-c}$, &c., all flow from this single principle. After establishing the different kinds of algebraic operations, and deducing the different forms of algebraic functions, it is found that $z+y\sqrt{-1}$ is a perfectly general algebraic expression, z and y being possible quantities, positive or negative. From these principles of abstract algebra we may proceed to use algebra as an instrument of research, that is, for finding from given data an unknown quantity. Being provided with rules of operation which are independent of relative magnitude, we can, if we call x the unknown quantity, operate upon it algebraically, and bring it into symbolic relation with the given quantities, without knowing its magnitude relative to theirs. But the unknown quantity acquires by these operations an algebraic expression, and must be included

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in the general form $z+y\sqrt{-1}$. It is, in fact, on this account that an equation not only gives by its solution the answer to a question which is possible, but also has a symbol to indicate that a question is impossible. It is true that, because of the generality of algebraic operations in respect to their being independent of relative magnitude, several algebraic functions may satisfy at the same time the final equation which denotes the relation of the unknown quantity to the given quantities. But these are all included in the same general form. Hence for the x of an equation we may always substitute $z+y\sqrt{-1}$, y vanishing for a possible root. These considerations would justify us in saying at the very beginning of the theory of equations, that the unknown quantity is $z+y\sqrt{-1}$, and that x is substituted for this expression for the sake of brevity. Unless this be understood to be the signification of x, the theory does not possess the requisite degree of generality. If this principle be admitted, the method of finding the impossible roots of an equation which I have indicated, is a necessary consequence. Mr. Airy will not move me from my position unless he points out an error or false principle in this *à priori* argument, or adduces a numerical instance that contradicts it.

Cambridge Observatory,
March 7, 1859.

XLVI. *On the Theory of Elliptically-polarized Light.* By Professor Challis*.

I VENTURED, in a former communication (Philosophical Magazine for January 1859), to advance the idea that a mathematical theory of physical forces may be based upon the dynamical action of a fluid medium pervading space, and so constituted that its pressure varies proportionally to its density. The theory of light which is generally advocated in the present day, assumes that space is occupied by a medium more resembling a solid than a fluid, and attributes the phænomena of light to the oscillations of its individual atoms. As the two media cannot be identical, or coexist, it is requisite, for the maintenance of my views, to inquire how far the phænomena of light are explained by the oscillatory theory; and in this inquiry I shall be compelled to canvass freely the views of its supporters. I have already shown, in the Philosophical Magazine for February, that that theory has hitherto failed to determine the direction of the transverse vibrations of a polarized ray, while the hydrodynamical theory decides on this point unequivocally. And even if

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the direction of the vibrations should eventually be determined by having recourse to phænomena of diffraction, the case of the oscillatory theory will not thereby be improved, because the usual mathematical treatment of that class of phænomena is more accordant with the hypothesis of a continuous fluid medium than with any other. I proceed now, with the same intention, to discuss the oscillatory theory of elliptically-polarized light; and in order to point out the actual state of this theory, I shall again refer to the able and useful address of Dr. Lloyd at the Dublin Meeting of the British Association in 1857.

After informing us of recent experiments by which it has been established that by reflexion "all bodies transform plane-polarized light into elliptically-polarized light, and impress a change of phase at the moment of reflexion," Dr. Lloyd adds as follows:—

"The theoretical investigations connected with this subject afford a remarkable illustration of one of those impediments to the progress of natural philosophy which Bacon has put in the foremost place among his examples of the *Idola*: I mean the tendency of the human mind to suppose a greater simplicity and uniformity in nature than exists there. The phænomena of polarization compel us to admit that the sensible luminous vibrations are *transversal*, or in the plane of the wave itself; and it was naturally supposed by Fresnel, and after him by MacCullagh and Neumann, either that no *normal* vibrations were propagated, or that, if they were, they were unconnected with the phænomena of light. We now learn that it is by them that the *phase* is modified in the act of reflexion, and that, consequently, no dynamical theory which neglects them, or sets them aside, can be complete."

I believe I am correct in asserting that mathematicians who have treated of the hypothetical medium from which transverse vibrations have been deduced, have shown that the constitution (isotrope) which conducts to transverse vibrations excludes any change of density, and therefore excludes any normal vibrations that can have connexion with the phænomena of light. Hence if experiment proves, as above stated, that normal vibrations have a real effect on phænomena of light, it follows that the hypothetical constitution of the luminiferous medium is contradicted by fact, and must therefore, by an acknowledged rule of philosophizing, be abandoned. It will be necessary, if the same course of investigation be persisted in, to invent another constitution of the medium, in order to meet the case of the coexistence of normal with transverse vibrations. As I am not aware that the molecular constitution of a medium having this property has hitherto been indicated, I think that I shall be justified in the endeavour to call attention to the theory of luminous rays,

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which I have advanced in the Cambridge Philosophical Transactions, vol. viii. part 3, and in the Philosophical Magazine for December 1852. I have there demonstrated the existence of axes of rectilinear transmission, and of simultaneous normal and transverse vibrations, on hydrodynamical principles, no other hypothesis being made respecting the medium than that the density varies as the pressure. Suppose a wave-ray of this theory to be incident on a reflecting surface, and the transverse vibrations to be resolved in directions perpendicular and parallel to the plane of reflexion. Now although we may not yet be able to determine by mathematical reasoning the precise manner in which the ray comports itself at the moment of reflexion, from experimental facts we have reason to say that the reflexion does not take place strictly at the surface of the medium, but that the path of the axis of the ray penetrates to a certain minute distance below the surface. Nothing is less unlikely, considering the geometrical relations of the directions of the vibrations to the superficies of the medium, than that this distance should be different for the two sets of vibrations, and that the observed change of phase is owing to this circumstance.

There is another question relating to this experiment which requires an answer. Why is the change of phase not apparent unless the incident ray be plane-polarized? The theory of polarization, founded on hydrodynamical principles, which I have proposed in the Cambridge Philosophical Transactions, vol. viii. part 3, gives the following explanation:—If a ray of common light be divided into two plane-polarized rays which pursue the same course with difference of phase, the compound ray is still equivalent to a ray of common light; but if a plane-polarized ray be similarly divided, the compound ray is circularly or elliptically polarized. The commonly received theory of polarization, which rests upon the hypothetical medium before referred to, is incapable of giving a like explanation, because it is incapable of making a distinction between common light and elliptically-polarized light.

Being desirous that the objections I advance against the oscillatory theory should rest on admissions or assertions made by its own supporters, in order to justify the objection just urged, I shall refer to a passage in Mr. Airy's 'Treatise on the Undulatory Theory of Light.' Near the end of the treatise the following statement occurs:—"Common light consists of successive series of elliptical vibrations (including in this term plane and circular vibrations), all the vibrations of each series being similar to each other, but the vibrations of one series having no relation to those of another. The number of vibrations in each series must amount to at least several hundreds; but the series must

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be so short that several hundred series enter the eye in every second of time." All that I am concerned with in this statement is, the evidence it gives that a separate hypothesis, in no manner related to the original hypothetical medium from which alone transverse vibrations were derived, has been found necessary to distinguish between common light and elliptically-polarized light. According to strict deduction from the hypothetical medium *there is no such distinction*; and consequently, as there is a distinction in fact, the same *regula philosophandi* which I referred to in speaking of normal vibrations, requires that the hypothetical medium be abandoned. Mr. Airy's supplementary hypothesis involves the necessity of indicating the constitution of a medium which has the property of producing these alternating series of transverse vibrations; but as this has not been done, I beg to call attention to the very simple account of the different kinds of light which is given by the hydrodynamical theory. In this theory common light consists of *quaquaversus* rectilinear transverse vibrations equal in all directions from the axis of the ray, plane-polarized light, of rectilinear transverse vibrations parallel to a plane, circularly and elliptically-polarized light of circular and elliptical transverse vibrations.

In a paper "On the Composition and Resolution of Streams of Polarized Light" (Camb. Phil. Trans. vol. ix. part 3), Prof. Stokes has adopted Mr. Airy's hypothesis, and argued that the transition from one series to the next may be gradual; but he has not indicated a constitution of the luminiferous medium which will help us to conceive of a *cause* for such transitions.

The success with which the oscillatory theory of light accounts for the phænomena of double refraction has been appealed to as evidence of its truth. But it is to be observed that there are two distinct classes of facts to be explained in physical optics; the nature and qualities of luminous rays considered by themselves, and the laws according to which they comport themselves when they come into contact with visible media. The latter class of phænomena are the more complex; and the treatment of them necessitates additional hypotheses respecting the molecular constitution of these media. In Fresnel's theory of double refraction, the hypotheses relating to the æther and the refracting media are so blended, that that theory is inappropriate to be a test of the constitution of the ætherial medium itself. In the Cambridge Philosophical Transactions, vol. viii. part 4, I have obtained the known equation of the wave-surface, on the hypothesis that the æther is a continuous medium of variable density and pressure, joined with certain other hypotheses relating exclusively to the molecular constitution of the doubly refracting substance.

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The tendency of recent theories of light has been to evade the difficulty respecting the constitution of the ætherial medium, by seeking to discover only a representation of the facts of light by analytical *formulæ*. But the grouping of facts under formulæ is not the same thing as comprehending them in a theory, which always implies the discovery of a *modus operandi*. A good theory of light ought to make known the characteristics of the medium through which the phænomena are produced.

Cambridge Observatory,
March 12, 1859.

XLVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 228.]

June 17, 1858.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Note on Sodium-ethyle and Potassium-ethyle." By Edward Frankland, Ph.D., F.R.S.

The recent interesting discovery of sodium-ethyle and potassium-ethyle by Mr. Wanklyn, led me to investigate the cause of the non-formation of these bodies by reactions analogous to those successfully used for the production of zinc-ethyle and similar organo-metallic compounds. In my earlier experiments upon the isolation of the organic radicals, I studied the action of potassium and sodium upon iodide of ethyle, and found that the latter compound was readily decomposed by either of the metals at a temperature of from 100° to 130° C. The separated ethyle was, however, transformed almost completely into hydride of ethyle and olefiant gas, whilst not a trace of potassium-ethyle or sodium-ethyle was produced. Mr. Wanklyn has since repeated this experiment with the addition of ether, and has obtained the same result as regards the non-formation of an organo-metallic compound.

The temperature at which sodium decomposes iodide of ethyle is much lower than that at which sodium-ethyle is broken up, consequently no explanation of the phenomenon can be obtained from this source. In his observations on the formation of ethyle*, Brodie mentions that iodide of ethyle is decomposed at 170° C. by zinc-ethyle; and it therefore occurred to me that sodium-ethyle, owing to its more powerful affinities, might effect the decomposition of iodide of ethyle at a lower temperature than that at which iodide of ethyle is decomposed by sodium; in which case the production of sodium-ethyle, by the action of sodium upon iodide of ethyle, would be an impossibility. Experiment completely confirmed this anticipation. A quantity of a strong solution of sodium-ethyle in zinc-ethyle was thrown up into a dry receiver filled with mercury, and an equal volume of pure iodide of ethyle added to it. Immediately on the mixture of the two liquids,

* Journal of the Chemical Society, vol. iii. p. 405.

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a lively effervescence set in, a considerable quantity of gas collected in the receiver, and a white deposit of iodide of sodium rendered the liquid thick and turbid. The reaction was complete in two or three minutes without the application of heat. An analysis of the gas, previously freed from the vapours of iodide of ethyle and zinc-ethyle, showed it to consist of equal volumes of hydride of ethyle and olefiant gas, mixed only with a mere trace of ethyle. This reaction may therefore be thus expressed:—

$$C_4H_5Na + C_4H_5I = NaI + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} + C_4H_4.$$

It is therefore evident that sodium-ethyle, and the remark no doubt applies also to potassium-ethyle, could not be obtained by the action of sodium upon iodide of ethyle, even if the decomposition of the latter could be effected at ordinary temperatures, since each particle of the organo-metallic compound being in contact with iodide of ethyle at the moment of its formation, would be instantly decomposed in the manner just described. That olefiant gas and hydride of ethyle, with mere traces only of ethyle, constitute the gaseous product of the decomposition of iodide of ethyle by sodium, is strong evidence that this formation and immediate decomposition of sodium-ethyle actually takes place. Sodium-ethyle thus stands in the same relation to iodide of ethyle as hydride of zinc does to hydriodic acid; and consequently all attempts to produce hydride of zinc by the action of the metal upon the hydrogen acids have failed. These considerations, taken in connexion with Mr. Wanklyn's mode of forming sodium-ethyle and potassium-ethyle, afford a clue to the nature of the reactions by which we shall probably eventually succeed in forming the hydrogen compounds of the highly positive metals. Although the hydrogen compounds of arsenic, antimony, phosphorus, and tellurium are by no means exact analogues of zinc-ethyle, it would nevertheless be interesting to ascertain the action of sodium upon these bodies, with a view to the formation of hydride of sodium.

The nature of the gas evolved by the action of sodium-ethyle upon iodide of ethyle, has some interest in connexion with the formation of ethyle by the action of zinc upon iodide of ethyle. Brodie expressed, in the memoir above alluded to, an ingenious and highly probable hypothesis, that the true source of the ethyle is the decomposition of its iodide by zinc-ethyle, thus:—

$$C_4H_5Zn + C_4H_5I = ZnI + \left.\begin{matrix} C_4H_5 \\ C_4H_5 \end{matrix}\right\};$$

and that the secondary products of the reaction (olefiant gas and hydride of ethyle) which always accompany the ethyle, result from the primary action of zinc upon iodide of ethyle, thus:—

$$2(C_4H_5I) + 2Zn = C_4H_4 + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} + 2ZnI.$$

The composition of the gases produced in the above reaction of sodium-ethyle upon iodide of ethyle seems, however, to indicate that the reverse of this hypothesis is true, and that the source of the ethyle is

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to be found in the primary action of zinc upon iodide of ethyle,—

$$2(C_4H_5I)+2Zn=\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}+2ZnI,$$

whilst the secondary products are derived from the decomposition of iodide of ethyle by zinc-ethyle,—

$$C_4H_5I+ZnC_4H_5=\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}+C_4H_4+ZnI.$$

"Experimental Inquiry into the Composition of some of the Animals fed and slaughtered as Human Food." By J. B. Lawes, Esq., F.R.S., F.C.S., and J. H. Gilbert, Ph.D., F.C.S.

After alluding to the importance of the chemical statistics of nutrition in relation to physiology, dietetics, and rural economy, and explaining that the branch of the subject comprehended in the present paper is that of *Animal Composition*, the authors proceed in the first place to state the general nature of their investigations, and the manner in which they were conducted.

To ascertain the quantitative relations, and the tendency of development, of the different parts of the system, the weights of the entire bodies, and of the several internal organs, also of some other separated parts, were determined in several hundred animals—oxen, sheep, and pigs.

To determine the ultimate composition, and in a sense the proximate composition also, of oxen, sheep, and pigs, and to obtain the results in such manner that they might serve to estimate the probable composition of the *Increase* whilst fattening, was a labour obviously too great to be undertaken with a large number of animals. Those selected were—a fat calf, a half-fat ox, a moderately fat ox, a fat lamb, a store or lean sheep, a half-fat old sheep, a fat sheep, a very fat sheep, a store pig, and a fat pig.

It is to the methods and the results of the analysis of these ten animals, to the information acquired as to the quantitative relation of the organs or parts in the different descriptions of animal, and their relative development during the fattening process, and to the application of the data thus provided, that the authors chiefly confine themselves in the present paper.

The analyses of the ten animals were planned to determine the actual and per-centage amounts—of water, of mineral matter, of total nitrogenous compounds, of fat, and of total dry substance—in the entire bodies, and in certain individual and classified parts of the animals. The water, and mineral matter, were for the most part determined in each internal organ, or other separated part. But, to confine the labour within reasonable limits, and to facilitate as far as possible the perception of the practical and economic application of the results, the other constituents enumerated are given in—

1st. The collective "carcass" parts; that is, the frame with its covering of flesh and fat, which comprise the most important portions sold as human food.

2nd. The collective "offal" parts; including the whole of the

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internal organs, the head, the feet, and, in the case of oxen and sheep, the pelt and hair or wool.

3rd. The entire animal (fasted live-weight).

Referring first to the composition of the "*collective carcass parts,*" it appeared, comparing one animal with another, that there is a general disposition to a rise or fall in the per-centage of *mineral matter,* with the rise or fall in that of the *nitrogenous compounds.* In fact, all the results tended to show a prominent connexion between the amount of the mineral matters and that of the nitrogenous constituents of the body.

Comparing the relative proportions of *fat,* and *nitrogenous compounds,* in the respective "carcasses," it appeared that, in every instance excepting that of the calf, there was considerably more of dry fat than of dry nitrogenous compounds. In the carcass of even the store or lean sheep, there was more than 1½ times as much fat as nitrogenous substance; and in that of the store or lean pig, twice as much. In the carcass of the half-fat ox, there was one-fourth more fat than nitrogenous matter; and in that of the half-fat sheep, more than twice as much.

Of the fatter animals, the carcass of the fat ox contained 2⅓ times, that of the fat sheep 4 times, and that of the very fat sheep 6 times as much fat as nitrogenous substance. Lastly, in the carcass of the moderately fat pig, there was nearly 5 times as much fatty matter as nitrogenous compounds.

From these facts it may be concluded, that in carcasses of oxen in reputed good condition, there will seldom be less than twice as much, and frequently nearly 3 times as much dry fat as dry nitrogenous substance. It may be presumed, that in the carcasses of sheep the fat will generally amount to more than 3, and frequently to 4 (or even more) times as much as the nitrogenous matters; and finally, that in the carcasses of pigs killed for fresh pork, there will seldom be as little as 4, and in those fed for curing more than 4 times as much fat as nitrogenous compounds.

The *fat* of the bones constituted but a small proportion of that of the entire carcasses; whilst the *nitrogen* of the bones amounted to a considerable proportion of the whole.

It appeared, that whilst the per-centage (in the carcasses) of both mineral and nitrogenous matters *decreased* as the animals matured, that of the fat very considerably *increased.* The increase in the per-centage of fat was much more than equivalent to the collective decrease in that of the other solid matters,—that is to say, as the animal matures, the per-centage in its carcass, of *total dry substance* —and especially of fat—much increases.

The carcass of the calf contained 62¼ per cent., that of the lean sheep 57⅓rd per cent., that of the lean pig 55⅓rd, and that of the half-fat ox 54 per cent. of water. In the carcass of the fat ox there were 45½ per cent., in that of the fat lamb 48⅔rds per cent., in that of the half-fat old sheep 49⅔rds per cent., in that of the fat sheep 39⅔rds per cent., in that of the very fat sheep only 33 per cent., and in that of the moderately fattened pig only 38½ per

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cent. of water. The bones of the carcasses contained a less proportion of water than the collective soft or edible portions.

It is inferred, that the average of carcasses of well-fattened oxen will contain 50 per cent., or rather more, of dry substance; that those of properly fattened sheep will contain more still—say 55 to 60 per cent.; those of pigs killed for fresh pork rather more than those of sheep; whilst the sides of pigs fed and slaughtered for curing will be drier still. Lamb-carcasses would seem to contain a smaller proportion of dry substance than those of either moderately fattened oxen, sheep, or pigs. Their proportion of bone was also comparatively high. Veal appeared to be the moistest of all. The carcass of the calf experimented upon, though the animal was considered to be well fattened, contained only 37¾ per cent. of dry substance. Its proportion of bone was also higher than in any of the other animals.

Next as to the composition of the *collective offal parts* (excluding the contents of stomachs and intestines), the results showed that in every case the per-centage of nitrogenous substance was greater, and that of the fat very much less, than in the collective carcass parts.

In oxen and sheep, the pelt, hair or wool, hoofs, stomachs, and intestines, taken together, contained a large proportion of the total nitrogen of the offal parts. The portions of the nitrogenous offal parts of these animals generally used for food, are, the head-flesh with tongue and brains, the heart, the liver, the pancreas, the spleen, the diaphragm, and sometimes the lungs. In the pig, the proportion of the nitrogenous offal generally eaten, is greater than in the other animals; but its proportion of fat is generally also greater.

With the higher per-centage of nitrogenous substance, and the less per-centage of fat, in the collective offal parts, they had invariably a less per-centage of total dry substance, and therefore more of water, than the collective carcass parts.

From the composition of the *entire bodies* of the animals analysed, it is estimated, that of *mineral matter*, the average amount, in *store* or *lean* animals, will probably be, in oxen 4½ to 5 per cent., in sheep 3 to 3½ per cent., and in pigs 2½ to 3 per cent. As an average estimate for the *mineral* matter in *fattened* animals, the results indicated 3½ to 4 per cent. in the live-weight of calves and oxen, 2½ to 2¾ per cent. in that of sheep and lambs, and 1¼ to 1¾ per cent. in that of pigs.

Of total *nitrogenous compounds*, there were in the fasted live-weight of the fat ox 14½ per cent., in that of the fat sheep 12¼ per cent., in that of the very fat one not quite 11 per cent., and in that of the moderately fattened pig about the same, namely, 10·87 per cent. The leaner animals analysed contained from 2 to 3 per cent. more nitrogenous substance than the moderately fattened ones.

The *Fat* formed the most prominent constituent of the dry or solid substance of the entire animal bodies. The fat calf alone, contained less total fat than total nitrogenous compounds. Of the other professedly fattened animals, the entire bodies of the fat ox

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and fat lamb contained about 30 per cent., the entire body of the fat sheep 35½ per cent., that of the very fat sheep 45¾ per cent., and that of the moderately fat pig 42¼ per cent. of dry fat.

The average composition of the six animals assumed to be well fattened, showed, in round numbers, 3 per cent. of mineral matter, 12½ per cent. of nitrogenous compounds, and 33 per cent. of fat, in their standing or fasted live-weight.

All the experimental evidence conspired to show, that the so-called "*fattening*" of the animals, was properly so designated. During the feeding or fattening process, the per-centage of the collective dry substance of the body considerably increased; and the fatty matter accumulated in much larger proportion than the nitrogenous compounds. The *increase* itself, must therefore show a less per-centage of nitrogenous substance (and of mineral matter also), and a higher one of both fat and total dry substance, than the whole body of the fattened animal.

The knowledge thus acquired of the composition of animals in different conditions of maturity, was next employed as a means of estimating the composition of the *increase* gained in passing from one given point of progress to another.

To this end, the composition of the animals analysed in the lean condition, was applied to the known weights of numbers of animals of the same description, assumed to be in a similar lean condition; and the composition of the fat animals analysed, was in like manner applied to the weights of the same series of animals after being fattened. Deducting the amount of the respective constituents in the lean animals, from that of the corresponding constituents in the fat ones, the *actual* amount of each constituent gained was determined. The weight of the gross increase being also known, its estimated *per-centage* composition was thus a matter of easy calculation. The composition of the *increase* of 98 fattening oxen, 349 fattening sheep, and 80 fattening pigs (each divided into numerous lots), was estimated in the manner indicated; and as a control, a statement is given of the composition of the *increase* of the single analysed fat pig, which, at the time it was put to fatten, corresponded in weight and other particulars very closely with the one analysed in the lean condition.

It is concluded, that the *increase* in weight of *oxen*, taken over six months or more of the final fattening period, may be estimated to contain from 70 to 75 per cent. of total dry substance; of which 60 to 65 parts will be fat, 7 to 8 parts nitrogenous substance, and 1 to 1½ mineral matter.

On the same plan of calculation, the final *increase* of *sheep*, feeding liberally during several months, will probably consist of 75 per cent., or more, of total dry substance; of this, 65 to 70 parts will be fat, 7 to 8 parts nitrogenous compounds, and perhaps 1½ part mineral matter.

The *increase* of *pigs*, during the final two or three months of feeding for fresh pork, may be taken at 70 to 75 per cent. total dry substance, 65 to 70 per cent. fat, 6 to 8 per cent. nitrogenous substance,

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and less than 1 per cent. of mineral matter. The increase over the last few months of high feeding, of pigs fed for curing, will doubtless contain a higher per-centage of both fat and total dry substance, and a lower one of both nitrogenous compounds and mineral matter, than that of the younger and more moderately fattened animal.

As a general result, it appears that about $\frac{4}{5}$ths of the gross increase in live-weight, of animals feeding liberally for the butcher, will be dry or solid matter of some kind. About $\frac{2}{3}$rds of the gross increase will be dry fat; only about 7 or 8 per cent. of the gross increase (and scarcely more than $\frac{1}{10}$th of the total dry substance) will be nitrogenous compounds; and seldom more than $1\frac{1}{2}$, and frequently less than 1 per cent. mineral matter.

In the case of most of the sheep, and of all the pigs, the composition of whose increase was estimated, the amounts of mineral matter, of nitrogenous compounds, of non-nitrogenous organic substance, of total dry substance, and sometimes of fat, which were consumed during the fattening period, were determined; so that the means are at command for studying the quantitative relation of the constituents estimated to be stored up in the increase, to those consumed in the food which produced it.

Taking first the proportion of each class of constituents stored up *for* 100 *of the same consumed*, it is concluded, that in the case of sheep, liberally fed on a mixed diet of dry and succulent food, the increase of the animal will perhaps generally carry off less than 3 per cent. of the consumed mineral matter—somewhere about 5 per cent. (varying according to the proportion in the food) of the consumed nitrogenous compounds, and about 10 parts of fat for 100 non-nitrogenous substance in the food; and lastly, that for 100 of collective dry substance of food consumed, there will be, in sheep, about 8 or 9 parts of dry matter in increase stored up.

The food of the fattening pig contained a much smaller proportion of indigestible woody fibre than that of the sheep; and it appeared that the pig appropriated to its increase a much larger proportion of the organic constituents of its food than the sheep. The average of the estimates for pigs, showed about 17 parts of dry substance of increase stored up, for 100 of collective dry matter of food consumed. For 100 of non-nitrogenous organic constituents in food, about 20 parts of fat were stored up. Of nitrogenous compounds, when the food consisted of about the usual proportions of the leguminous seeds and cereal grains, from 5 to 7 or 8 parts were stored up for 100 consumed. When the leguminous seeds predominated, the *proportion* of the consumed nitrogen stored up was less; and when the cereal grains predominated, it was greater. The estimates showed, that on the average of the cases, there were 4 or 5 times as much fat stored up in increase, as there was of fatty matter supplied in the food. There was obviously therefore a *formation* of fat in the animal body.

Reckoning the amount of the *respective constituents* of increase stored up, *for* 100 *of the collective dry substance of the food consumed*, the general result was as follows:—It appeared, that of the

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about 9 parts of dry increase, in sheep liberally fed on corn or oil-cake and succulent roots, for 100 of dry food consumed, about 8 parts were non-nitrogenous substance, that is, *fat*. There was therefore only about 1 part stored as nitrogenous and mineral matters taken together. The average of the estimates showed the produce of 100 of the collective dry substance of the consumed food of sheep to be—about, 0·2 part of mineral matter, 0·8 part nitrogenous compounds, and 8 parts fat, stored up; leaving therefore about 91 parts to be expired, perspired, or voided.

Taking the average of all the estimates of this kind relating to *pigs*—of the $17\frac{1}{4}$ parts of dry increase for 100 of dry matter of food consumed, about $15\frac{3}{4}$ parts were estimated as fat, rather more than $1\frac{1}{3}$rd part nitrogenous substance, and an insignificant amount as mineral matter. On this plan of calculation, therefore, there would appear to be, in the case of fattening pigs, only from 82 to 83 parts of food-constituents expired, perspired, or voided, for 100 of the collective dry substance of food consumed.

It is obvious that the ultimate composition of the dry substance of increase must be very different from that of the 100 of dry substance consumed. This is strikingly illustrated in the case of the fat. In most of the experiments with pigs, the fatty matter in the food was determined. On the average of the cases it amounted to less than $\frac{1}{4}$th as much as was estimated to be stored up in the increase of the animals. There was obviously therefore a *formation of fat in the body* from some other constituent or constituents of the food. Supposing the $\frac{3}{4}$ths or more of the stored-up fat which must have been formed in the body to have been produced from *starch*, it was estimated that it would require $2\frac{1}{2}$ parts of starch to contribute 1 part of produced fat. Accordingly, it would appear that a much larger proportion of the consumed dry matter is, as it were, directly engaged in the production of the dry fatty increase, than is represented by the amount of the dry increase itself.

Thus, taking the average of the cases in which the fatty matter in the food of the pigs was determined, it was estimated that 17·4 parts of dry increase were produced for 100 of dry matter of food consumed. Of the 17·4 parts of dry increase, 16·04 are reckoned as fat. But there were only 3·96 parts of ready-formed fatty matter supplied in the food. At least 12·08 parts of fat must therefore have been produced from other substances. If from starch, it would require (at the rate of $2\frac{1}{2}$ parts of starch to 1 of fat) 30·2 parts of that substance for the formation of the 12·08 parts of produced fat. The ready-formed fat and the starch, together, thus supposed to contribute to the 16·04 parts of fat in the increase, would amount to 34·16 parts out of the 100 of dry matter of food consumed. But there were, further, 1·36 part of nitrogenous and mineral matters stored up in the increase. In all, therefore, 35·52 parts out of the 100 of gross dry matter consumed, contributed, in this comparatively direct manner, to the production of the 17·4 parts of gross dry increase.

According to the illustration just given, it appears that there was

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pretty exactly twice as much of the dry substance of the food, involved in the direct production of the increase, as there was of dry increase itself; hence instead of there being, as before estimated, 82 to 83 parts of the consumed dry matter expired, perspired, or voided, without as it were being directly involved in the production of the increase, it is to be inferred that, in the sense implied, only about 65 parts were so expired, perspired, or voided.

It having been thus found that by far the larger proportion of the solid increase of the so-called fattening animals is really *fat itself*,—as moreover, it is probable that, at least in great part, the fat formed in the body is normally derived from starch, and other non-nitrogenous constituents of the food—and since the current fattening foods contain such a very large amount of nitrogen compared with that eventually retained in the increase—it can hardly be surprising that, contrary to the usually accepted opinions, the comparative values of our staple food-stuffs are much more nearly measurable by their amount of digestible and assimilable non-nitrogenous constituents, than by that of the digestible and assimilable nitrogenous compounds.

In order to determine the relative development of the several organs and parts in different descriptions of animals, and in animals of the same description in different conditions of growth and maturity, the weights alive, and of the separate internal organs and some other parts, of 16 calves, heifers and bullocks, of 249 sheep, and of 59 pigs, were taken.

It appeared that in oxen the stomachs and contents constituted about 11½, in sheep about 7½, and in the pig only about 1¼ per cent. of the entire weight of the body. The amounts of the intestines and their contents stood in the opposite relation. They amounted in the pig to about 6¼, in the sheep to about 3½, and in the oxen to only about 2¾ per cent. of the whole body. These facts are of considerable interest, when it is borne in mind that in the food of the ruminant there is so large a proportion of indigestible woody fibre, and in that of the well-fed pig a comparatively large proportion of starch—the primary transformations of which are supposed to take place chiefly after leaving the stomach, and more or less throughout the intestinal canal.

Taken together, the stomachs, small intestines, large intestines, and their respective contents, constituted, in oxen more than 14 per cent., in sheep a little more than 11 per cent., and in pigs about 7⅓ per cent. With these great variations in the proportion in the different descriptions of animals, of these receptacles and first laboratories of the food (with their contents), the further elaborating organs, if we may so call them (with their fluids), appear to be much more equal in their proportion in the three cases. This is approximately illustrated in the fact, that taking together the recorded per-centages of "heart and aorta," "lungs and windpipe," "liver," "gall-bladder and contents," "pancreas," "milt or spleen," and the "blood," the sum indicated is for the oxen about 7 per cent., for the sheep about 7¼ per cent., and for the pigs about 6⅔rds per cent. Exclu-

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ding from this list the blood, which was more than ½rd of a per cent. lower in amount in the pigs than in the other animals, the sums of the per-centages of the other parts enumerated would agree even much more closely for the three descriptions of animal.

With regard to the influence of progression in maturity and fatness of the animal, upon the relative development of its several parts, the results showed, that the internal organs and other offal-parts pretty generally *increased* in *actual weight*, as the animals passed from the lean to the fat, or to the very fat condition. The *per-centage proportion* to the whole live-weight, of these offal-parts, as invariably *diminished* as the animals matured and fattened. The carcasses, on the other hand, invariably increased, not only in *actual* weight, but in *proportion* to the whole body.

The conclusion is, that in the feeding or fattening of animals, the apparatus which subserves for the reception and elaboration of the food does not increase commensurately with those parts which it is the object of the feeder to store up from that food. These parts are comprised in the "carcass" or frame-work, with its covering of flesh and fat. Of the carcasses which thus constitute the greater part of the increase, the nitrogenous portions increase but little, whilst the *fat* does so in very much larger proportion. Of the internal parts, again, it is also the *fat* which increases most rapidly.

The maturing process consists, then, in diminishing the proportional amount in the whole body, of the collective muscles, tendons, vessels, fleshy organs, and gelatigenous matters—the motive and functional, or so to speak, working parts of the body—the constituents of which alone, can increase the amount, or replace the transformed portions, of similar matters in the human body. It consists, further, in increasing very considerably the deposition of *fat*—one of the *non*-flesh-forming, but most concentrated of the respiratory and fat-storing constituents of human food.

It is then in our *meat-diet*, of recognized good quality, to which is generally attributed such a relatively high *flesh-forming* capacity, that we carefully store up such a large proportion of *non*-flesh-forming, but concentrated respiratory material.

One of the most important applications which can be made of a knowledge of the composition of the animals which constitute the chief sources of our animal food, is to determine the main points of distinction between such food and the staple vegetable substances which it substitutes or supplements in an ordinary mixed diet.

By the analysis of some of the most important animals fed and slaughtered as human food, it was found that the *entire bodies*, even when in a reputed lean condition, may contain more dry fat than dry nitrogenous substances. Of the animals "ripe" for the butcher, a bullock and a lamb contained rather more than twice, a moderately fat sheep nearly three times, and a very fat sheep and a moderately fat pig about four times as much dry fat as dry nitrogenous matter. Of the professedly fattened animals analysed, a fat calf alone contained rather less fat than nitrogenous compounds.

It was estimated, that of the whole *nitrogenous substances* of the

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body, 60 per cent. in the case of calves and oxen, 50 per cent. in lambs and sheep, and 78 per cent. in pigs, would be consumed as human food. Of the total *fat* of the bodies, on the other hand, it was supposed, that in calves and lambs 95 per cent., in oxen 80 per cent., in sheep 75 per cent., and in pigs 90 per cent. would be so applied.

Assuming the proportional consumption of the fat and nitrogenous compounds to be as here estimated, there would be, in the fat calf analysed 1½ time, in the fat ox 2¾ times, in the fat lamb, fat sheep, and fat pig nearly 4½ times, and in the very fat sheep 6¼ times as much dry fat as dry nitrogenous or flesh-forming constituents consumed as human food.

It would perhaps be hardly anticipated, that in the staple of our meat-diet, to which such a high relative flesh-forming capacity is generally attributed, there should be found such a high proportion of non-flesh-forming to flesh-forming matter as above indicated. The result of such a comparison as present knowledge permits in regard to the same point between the staple of our animal food and the more important kinds of vegetable food, will certainly not be less surprising.

Of the staple *vegetable* foods, *wheat-flour bread* is, at least in this country, the most important. It will be interesting, therefore, to contrast with this substance the estimated consumed portions of the analysed animals. To this end some assumption must be made as to the relative values (on the large scale), for the purposes of respiration and fat-storing, of the starch and its analogues in bread, and of the fat in meat. It is assumed that, in round numbers, 1 part of fat may be considered equal to 2½ parts of starch in these respects. If, therefore, the quantity of fat in the estimated consumed portions of the analysed animals be multiplied by 2·5, it is brought to what may be conveniently called its "*starch-equivalent*;" and in this way, the Meat and the Bread can be easily compared with one another, in regard to the relation of their flesh-forming, to their respiratory and fat-forming capacities.

Reckoning the amount—say 1 per cent.—of fat in Bread itself (and it probably averages not more than ½ per cent.), to be equal to 2½ parts of starch, and adding this to the amount of the actual starch and allied matters which it on the average contains, the calculation gives—assuming this *starch-equivalent* to represent specially the respiratory and fat-forming, and the nitrogenous substances the flesh-forming matter—6·8 parts of respiratory and fat-forming, to 1 of flesh-forming material, in *Bread*.

Taking the relation of the one class of constituents to the other, in the estimated total consumed portions of the animals assumed to be in fit condition for the butcher, there was only one case—that of the fat calf—in which the proportion of the so measured respiratory and fat-forming, to the flesh-forming capacity, was in this our meat-diet, lower than in Bread. In the estimated total consumed portions of the fat ox, the proportion of the *starch-equivalent* of non-flesh-forming matter to 1 of nitrogenous compounds, was 6·9, or rather

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higher than in Bread. In the estimated consumed portions of the fat lamb, the fat sheep, and the fat pig, the proportion was more than 1½ time as great as in Bread; and in those of the extra fat sheep it was more than twice as great. Taking the average of the 6 cases, there were nearly 10 parts of *starch-equivalent* to 1 of nitrogenous compounds, against 6·8 to 1 in Bread. In the half-fat ox, and the half-fat old sheep, neither of which were in the condition of fatness of such animals as usually killed, the relation of the *starch-equivalent* to the nitrogenous compounds (assuming only the same proportion of the total fat as before to be eaten), was in the former considerably, and in the latter slightly lower than in Bread, namely, as 3·83 to 1 in the half-fat ox, and as 6·28 to 1 in the half-fat old sheep.

It will perhaps be objected, that, when animals are so far fattened as to attain the relations above stated, the feeder is simply inducing disease in the animals themselves, and frustrating that which, it is considered, should be the special advantage of a meat-diet, namely, the increase in the relative supply of the flesh-forming constituents in our food. It cannot be doubted, however, that in animals that would be admitted, by both producer and consumer, to be in only a proper condition of fatness, there would be a higher relation of non-nitrogenous substance (so far as its respiratory and fat-forming capacity is concerned), to flesh-forming material, in their total consumed portions, than in the average of our staple vegetable foods. It may be true, that with the modern system of bringing animals very early forward, the development of fat will be greater, and that of the muscles and other nitrogenous parts less, than would otherwise be the case; but it is certain, that if meat is to be economically produced, so as to be within the reach of the masses of the population, it can only be so on the plan of early maturity. Nor will it be questioned, that the admixture with their otherwise vegetable diet, of the meat so produced, is, in practice, of great advantage to the health and vigour of those who consume it.

It is true that individual joints or other parts, as sold, will frequently have a less proportion of fat to flesh-forming matter than, according to the above supposition, will be consumed. Some fat will also be removed in the process of cooking. But this portion will generally still be consumed in some form. And where fresh meat is bought, so also are suet, lard, and butter, which either add to the fatness of the cooked meats, or are used further to reduce the relative flesh-forming capacity of the collaterally consumed vegetable foods.

It would, indeed, appear to be unquestionable, that the influence, on the large scale, of the introduction of animal food to supplement our otherwise mainly farinaceous diet, is to reduce, and not to increase, the relation of the nitrogenous or peculiarly flesh-forming, to the non-nitrogenous constituents (reckoned in their respiratory and fat-forming capacity), of the food consumed.

That, nevertheless, a diet containing a due proportion of animal food is, for some reason or other, generally better adapted to meet

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the collective requirements of the human organism than an exclusively bread or other vegetable one, the testimony of common experience may be accepted as sufficient evidence. Whatever may prove to be the exact explanations of the benefits arising from a mixed animal and vegetable diet, it is at any rate pretty clear, that, independently of any difference in the physical, and perhaps even chemical relations of the nitrogenous compounds, they are essentially connected with the amount, the condition, and the distribution of the *fat*, in the animal portions of the food.

Fat is the most concentrated respiratory, and of course fat-storing material also, which our food-stuffs supply. It cannot be doubted that, independently of the mere supply of constituents, the conditions of concentration, of digestibility, and of assimilability, of our different foods, must have their share in determining the relative values, for the varying exigences of the system, of substances which, in a more general, or more purely chemical sense, may still justly be looked upon as mutually replaceable.

By the aid of Chemistry, it may be established, that, in the admixture of animal food with bread, the relation (in respiratory and fat-forming capacity) of the non-flesh-forming to the flesh-forming substances, will be increased;—and further, that in such a mixed diet, the proportion of the non-flesh-forming constituents, which will be in the concentrated form, so to speak, of *fat itself*, will be considerably greater than in bread alone. Common experience also testifies to the fact of advantages so derived. It remains to Physiology to lend her aid, to the full explanation of that which Chemistry and common usage have thus determined.

"Note on the Formation of the Peroxides of the Radicals of the Organic Acids." By B. C. Brodie, F.R.S.

The researches of Gerhardt showed the close resemblance which exists between the monobasic organic acids and the metallic protoxides. We have the chloride of acetyle corresponding to the chloride of the metal, and the hydrated and anhydrous acetic acid corresponding to the hydrated and anhydrous oxide. These investigations have been succeeded by others, which have had their origin in the consistent development of these ideas. The following discovery extends and completes these analogies. I have to add a new term to this series, of which hitherto no analogue has existed. This term is the peroxide of the organic radical,—the body which in the series of acetyle corresponds to the peroxide of hydrogen or barium in the series of the metal. Of these remarkable substances I have prepared two,—the peroxides of benzoyle and of acetyle; but the method by which these are procured is doubtless of extensive application, and we may consider ourselves as in possession of a class of bodies of a new order, the study of which cannot fail greatly to extend our knowledge.

These peroxides are prepared by the action of the anhydrous acid, or the corresponding chloride, upon the peroxide of barium. It is first necessary to prepare this peroxide in a pure condition. This is effected by precipitation of the solution of the peroxide of barium in

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hydrochloric acid by baryta-water, and by drying *in vacuo* the precipitate thus obtained. The peroxide of barium thus procured is perfectly pure, with the exception of a trace of carbonate. In appearance it resembles magnesia.

To prepare the peroxide of benzoyle, the chloride of benzoyle and the peroxide of barium are taken in equivalent proportions and mixed in water. A mutual decomposition takes place; and a substance is formed which, after crystallization from anhydrous ether, gave the following results to analysis:—

Carbon	69·23
Hydrogen	4·10
Oxygen	26·67
	100·00

The calculated numbers for the peroxide of benzoyle are

C_{14}	168	69·42
H_{10}	10	4·13
O_4	64	26·45
	242	100·00

This substance contains an atom of oxygen more than the anhydrous acid, and (reducing the formula to its simplest expression) one atom of hydrogen less than the hydrated acid. Thus we have $C_{14} H_{10} O_3$ anhydrous benzoic acid, $C_{14} H_{10} O_4$ peroxide of benzoyle, and $C_7 H_6 O_2$ hydrated benzoic acid, $C_7 H_5 O_2$ peroxide of benzoyle, as we have H_2O water, and $H_2 O_2$ or HO for the peroxide of hydrogen. This body crystallizes from ether in large and brilliant crystals. Heated a little above the boiling-point of water, it decomposes, with a slight explosion and the evolution of carbonic acid. Boiled with a solution of potash, it is resolved into oxygen gas and benzoic acid.

The peroxide of acetyle is prepared by mixing anhydrous acetic acid and peroxide of barium, in equivalent proportions, in anhydrous ether. The mixture is to be effected very gradually, being attended with evolution of heat. The ether, after filtration from the acetate of baryta produced, is to be carefully distilled off at a low temperature, and the fluid which remains washed with water. After three or four washings, the water ceases to be acid, and a viscid liquid remains, which is the peroxide of acetyle. This substance possesses the following properties:—It is extremely pungent to the taste; the smallest portion of it placed upon the tongue burns like cayenne pepper. The substance suspended in water immediately decolorizes a solution of sulphate of indigo. It instantly peroxidizes the protoxide of manganese, and converts the yellow prussiate of potash into the condition of red prussiate. Baryta-water poured upon the substance is converted to the condition of peroxide of barium, with formation of acetate of baryta. Lastly, a single drop of the substance itself, placed on a watch-glass and heated, explodes with a loud report, shivering the glass to atoms.

To analyse the peroxide of acetyle, I availed myself of its decom-

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position by baryta-water. An undetermined quantity of the substance was thus decomposed, and the oxygen estimated which was evolved by the decomposition of the peroxide of barium formed, by platina-black, and the acetate of baryta determined as sulphate. The result is the same as though the peroxide of acetyle were decomposed into anhydrous acetic acid and oxygen, thus,

$$C_4 H_6 O_4 = C_4 H_6 O_3 + O.$$

Thus for every 16 parts of oxygen evolved, 2 equivalents of acetate of baryta and 1 of sulphate of baryta, $SO_4 Ba_2$, would be produced. Now we have

$$\begin{array}{cccccc} SO_4 Ba_2 & & O & & & \\ 233{\cdot}2 & : & 16 & :: & 100 & : 6{\cdot}86. \end{array}$$

In the actual experiment 1·776 gr. of sulphate of baryta was obtained, and 0·1225 of oxygen evolved.

$$1{\cdot}776 : 0{\cdot}1225 :: 100 : 6{\cdot}89.$$

It has not yet been in my power to pursue further the study of these substances. I may, however, observe, that the peroxide of acetyle contains the elements of carbonic acid and of the acetate of methyle, and the peroxide of benzoyle the elements of carbonic acid and of the benzoate of phenyle. I have ascertained that the peroxide of benzoyle, when carefully heated, loses exactly one equivalent of carbonic acid; but the substance formed, although isomeric with the benzoate of phenyle, has not the properties of that body. It is a yellow resin, soluble in ether and alkalies, from which latter solution it is precipitated by acids.

The existence of a hydrated peroxide may be anticipated, intermediate between the organic peroxide and the peroxide of hydrogen, in the same sense as the organic acid is intermediate between water and the anhydrous acid. This substance in the series of benzoyle would be isomeric with salicylic acid. My efforts, however, to procure these bodies have, as yet, been unsuccessful; and it is to be remembered that we have no evidence of the existence of a hydrated peroxide of barium, or of any other metal, corresponding to the hydrated protoxide. In the series of ethyle the diatomic alcohol of Wurtz ($C_2 H_6 O_2$) is isomeric with the hydrated peroxide. But the true peroxide of ethyle remains yet to be discovered.

The question naturally arises as to what would be the result of making similar experiments with the chlorides and the anhydrides of the bibasic acids. Now carbonic acid may be regarded as the peroxide of oxalic acid: it is the constant product of the action of oxidizing agents upon that body; and were we able to procure the unknown anhydride of oxalic acid, it would not be an unreasonable anticipation that with the peroxide of barium it would decompose into oxalate and carbonic acid, thus

$$2 C_2 O_3 + Ba_2 O_2 = C_2 O_4 Ba_2 + 2 CO_2.$$

A similar experiment with anhydrous succinic acid would produce succinate of baryta and a homologue of carbonic acid, the existence

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of which is also indicated by other considerations. It is premature to dwell upon this point; but in this direction also I have made some experiments.

"Notice of Researches on the Sulphocyanide and Cyanate of Naphtyle, conducted by Vincent Hall, Esq." By A. W. Hofmann, Ph.D., F.R.S. &c.

The transformation of phenylcarbamide and phenylsulphocarbamide under the influence of anhydrous phosphoric acid, respectively into cyanate and sulphocyanide of phenyle, an account of which I submitted to the Society several months ago, suggested the probability that the hitherto unknown cyanates and sulphocyanides of radicals similar to phenyle might be obtained by analogous processes.

To establish this point experimentally, Mr. Vincent Hall has examined, in my laboratory, the deportment of some of the derivatives of *naphtylamine* under the influence of agents capable of fixing ammonia and its analogues.

Mr. Hall has found that the crude naphtaline, such as it is obtained from the gas-works, submitted at once, without sublimation, to the action, first of fuming nitric acid, and subsequently of acetic acid and metallic iron, furnishes the naphtylamine sufficiently pure for these experiments. The crude product thus obtained was digested with bisulphide of carbon in order to convert it into *naphtylsulphocarbamide.*

By distilling naphtylsulphocarbamide with anhydrous phosphoric acid, Mr. Hall has obtained a beautiful crystalline compound of a faint but peculiar odour, readily fusible, easily soluble in alcohol and ether, insoluble in water.

The analysis of this compound has led to the formula

$$C_{22}H_7NS_2 = C_{20}H_7, C_2NS_2,$$

showing that it is in fact *sulphocyanide of naphtyle*, formed according to the equation:—

$$\left.\begin{matrix}(C_2\ S_2)''\\ (C_{20}\ H_7)_2\\ H_2\end{matrix}\right\} N_2 = C_{20}\left.\begin{matrix}H_7\\ H_2\end{matrix}\right\} N + C_{20}H_7, C_2NS_2.$$

Boiled with an alcoholic solution of naphtylamine, this compound readily reproduces naphtylsulphocarbamide, which by its insolubility is easily distinguished and separated from the sulphocyanide.

Gently heated with phenylamine, the new sulphocyanide gives rise to the formation of a crystalline compound, of properties very similar to those of the naphtylsulphocarbamide. This new body is *phenyl-naphtyl-sulphocarbamide**, containing—

$$C_{34}H_{14}N_2S_2 = C_{12}H_5, \left.\begin{matrix}(C_2\ S_2)''\\ C_{20}\ H_7\\ H_2\end{matrix}\right\} N_2.$$

* By the action of sulphocyanide of phenyle upon naphtylamine, I have obtained a crystalline compound very similar in its general characters to the body which Mr. Hall procures by the action of sulphocyanide of naphtyle on phenyla-

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Naphtylcarbamide, as obtained by the action of potassa on the corresponding sulpho-compound, or by the distillation of oxalate of naphtylamine, is likewise powerfully attacked by anhydrous phosphoric acid. Among the products of distillation a compound is found, which, by its chemical properties, is readily identified as *cyanate of naphtyle*,

$$C_{22}H_7NO_{20}=C_2H_7, C_2NO_2,$$

although the small quantity in which this body is produced—by far the greater amount of the naphtylcarbamide being charred by the action of anhydrous phosphoric acid—has hitherto prevented Mr. Hall from fixing the nature of the compound by an analysis.

"Preliminary Account of an Inquiry into the Functions of the Visceral Nerves, with special reference to the so-called 'Inhibitory System.'" By Joseph Lister, Esq., F.R.C.S. Eng. & Edin.

November 18, 1858.—Richard Owen, Esq., V.P., in the Chair.

The following communications were read:—

"On the Theory of the Vertebrate Skull." [The Croonian Lecture.] By Thomas H. Huxley, Esq., F.R.S.

"On the Changes produced in the proportion of the Red Corpuscles of the Blood by the administration of Cod-Liver Oil." By Theophilus Thompson, M.D., F.R.S.

"Further Observations on the Power exercised by the Actiniæ of our Shores in killing their prey." By R. M'Donnell, M.D.

"On the Digestive and Nervous Systems of *Coccus hesperidum*." By John Lubbock, Esq., F.R.S., F.L.S., F.G.S.

GEOLOGICAL SOCIETY.

[Continued from p. 231.]

February 23, 1859.—L. Horner, Esq., in the Chair.

The following communications were read:—

1. "On the occurrence of Liassic Deposits near Carlisle." By E. W. Binney, Esq., F.G.S.

The author's attention had been drawn by Mr. Richard B. Brockbank, of Carlisle, to the district lying between Carthwaite, on the Carlisle and Maryport Railway, and the Solway, especially about Aikton and Oughterby, as containing a limestone, supposed to belong to the coal-measures, but found by Mr. Brockbank to contain an Ammonite and other fossils, which he thought to be Liassic. Mr.

mine. This substance likewise contains

$$C_{34}H_{14}N_2S_2=C_{12}H_5, C_{20}H_7\left\{\begin{matrix}(C_2S_2)''\\H_2\end{matrix}\right\}N_2,$$

for

$$C_{12}H_5H_2N+C_{20}H_7C_2NS_2=C_{20}H_7, H_2N+C_{12}H_5, C_2NS_2=C_{34}H_{14}N_2S_2.$$

Are these two bodies identical, or only isomeric? [A.W.H.]

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Binney subsequently went over the district with Mr. R. B. Brockbank, and found that, although the country is thickly coated with boulder-clay or till, yet lias-limestone and shales were observable in several spots, in wells, streams, &c., especially at Quarry Gill, Fisher's Gill Farm, and in Thornbybrook, south-east of Aikton. *Gryphæa incurva* and other *Gryphææ*, with Oysters and Ammonites, characterize these beds. The area occupied by the Lias is known to extend under the rising ground lying between Crofton and Orton, on the south, and the Solway, on the north, comprising Aikton, Thornby, Wiggonby, Oughterby, and probably other places on the rising ground between the Carlisle and Maryport, and Carlisle and Port Carlisle Railways.

2. "On the Fossils of the Lingula-flags or Zone Primordiale. —I. Paradoxides and Conocephalus from North America." By J. W. Salter, Esq., F.G.S., of the Geological Survey of Great Britain.

After briefly noticing the relations of the "Zone Primordiale" instituted by M. Barrande, the author described the remains of a large *Paradoxides* sent from the vicinity of St. John's, Newfoundland, by Mr. Bennett. The fossil belongs to a new species of *Paradoxides*, the largest yet known (9½ inches broad), and termed *P. novo-repertus* by Mr. Salter. A new species of *Conocephalus*, from Georgia, was also described from a specimen brought to England by Dr. Feuchtwanger, and placed in the Great Exhibition of 1851; it is named *C. antiquatus* by the author. As these two genera have as yet been known only in the "Zone Primordiale," Mr. Salter regards the above-mentioned specimens as indicative of the existence of that geological formation in the countries here mentioned.

The author also referred to an obscure specimen of *Asaphus*, from the "Calciferous sand-rock" of Canada, which he once, but on insufficient grounds, published as a *Paradoxides*.

3. "On a new species of Dicynodon (*D. Murrayi*) from near Colesberg, South Africa." By Prof. T. H. Huxley, F.R.S., Sec. G.S.

For the original specimen from which Prof. Huxley first obtained (in the spring of last year) evidence of the existence of this species, he was indebted to the Rev. H. M. White, of Andover, who subsequently put the author in communication with the discoverer of the fossil, Mr. J. A. Murray; and the latter gentleman having written to his father, resident in South Africa, obtained for Prof. Huxley a large quantity of similar fossil remains. One specimen in particular, having been carefully chiselled out by Mr. Dew, afforded a complete skull of this peculiar and previously undescribed species of *Dicynodon*.

The author described the distinctive features of this skull in detail. *Dicynodon Murrayi* is distinguished from all the already known species by the following characters:—

1. The plane of the upper anterior face of the nasal and premaxillary bones would, if produced, cut that of the upper face of the parietal at an angle of about 90°.

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2. The supratemporal fossæ are much longer from within outwards than from before backwards, owing partly to the shortness of the parietal region.

3. The alveoli of the tusks, the transverse section of which is circular, commence immediately under the nasal aperture, and extend forwards and downwards parallel with the plane of the nasal and upper part of the premaxillary bones, and do not leave their sockets until they have passed beyond the level of the posterior end of the symphysis of the lower jaw.

4. The nasal apertures are altogether in front of the orbits.

5. The length of the upper jaw in front of the nasal apertures is certainly equal to one-third, and probably to one-half, the whole length of the skull, which is between 6 and 7 inches.

6. The os quadratum is about half as long as the skull.

These peculiarities are regarded as sufficient to distinguish *Dicynodon Murrayi* from all others; and the author stated that he should reserve the description of many other anatomical features, which are probably more or less common to other Dicynodons, such as the bony sclerotic, the bony interorbital septum and vomer, the characters of the humerus, of the pelvis, and of the ribs, for another paper, in which other Dicynodont remains will be considered.

4. "On the Coal found by Dr. Livingstone at Tete, on the Zambesi, South Africa." By Richard Thornton, Esq.

Mr. Thornton states that this coal is free-burning; showing no tendency to cake; containing very little of either sulphur or iron, a large proportion of ash, but only a little gaseous matter. The result of the trial (made in the steam-launch) of this coal, and its appearances, favour, in the author's opinion, the idea that the coal, when taken from a deeper digging (that which Dr. Livingstone had sent was collected at the surface of the ground), will probably contain less ash and a little more gaseous matter.

March 9, 1859.—Sir C. Lyell, Vice-President, in the Chair.

The following communications were read:—

1. "On some Minerals from Persia." By the Hon. C. A. Murray.

The mineral specimens referred to were obtained from the district between Tabriz and the Caspian, especially from the Karadagh Range, and consist of native copper, chrysocolla, red oxide and black oxide of copper, malachite, azure-copper, bornite, copper-glance, copper-pyrites, varieties of galena, zinc-blende, magnetite, specular iron-ore, manganese-ore, orpiment, sulphur, and brown-coal. The series of copper-ores appears to indicate the existence of considerable masses of metallic mineral, probably in lodes or regular veins. The lead-ores have the appearance of having been taken either from veins of small size, or from near the surface of the ground.

2. "On the Veins of Tin-ore at Evigtok, near Arksut, Greenland." By J. W. Tayler, Esq., F.G.S.

These tin-veins, of which there are about twenty, extend over an area of about 1300 feet in length by 80 feet in breadth; and they

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run in various directions, some E. and W., others N.E. and S.W., and others N. and S. They vary from 10 inches to ¼ of an inch in width; in the largest veins the tin-ore occupies about 1 inch of one side of the vein. The veins nearly all occur in a great vein of felspar and quartz; which contains also ores of lead, copper, zinc, iron, and molybdena, associated with cryolite, fluor-spar, zircon, &c.

3. "On the Permian Chitonidæ." By J. W. Kirkby, Esq. Communicated by T. Davidson, Esq., F.R.S., F.G.S.

After having fully noticed the progress of our knowledge respecting the palæozoic Chitons, and those of the Magnesian Limestone in particular, the author described in detail the characters of *Chiton Loftusianus*, King, and *Chiton Howseanus*, Kirkby, and a new species, referred with some doubt to *Chiton, C.* (?) *cordatus*; also *Chiton antiquus*, Howse, which Mr. Kirkby refers to the subgenus *Chitonellus*, as well as two new species, *C. Hancockianus* and *C. distortus*. The specimens on which all these species have been determined have been found in the Magnesian Limestone of the neighbourhood of Sunderland, Durham, and chiefly in that of Tunstall Hill.

The author particularly alluded to the great similarity that some of the plates of these fossil Chitons have at first sight to *Patellæ* and *Calyptrææ*, and recommended that especial care should therefore always be taken in the determination of patelliform fossils.

4. "On the Vegetable Structures in Coal." By J. W. Dawson LL.D., F.G.S., Principal of M'Gill College, Montreal.

After referring to the labours of others in the elucidation of the history of coal, the author remarks that in ordinary bituminous coal we recognize by the unaided eye laminæ of a compact and more or less lustrous appearance, separated by uneven films and layers of fibrous anthracite or mineral charcoal. As these two kinds of material differ to some extent in origin and state of preservation, and in the methods of study applicable to them, he proceeds to treat of his subject under two heads:—1st. The structures preserved in the state of mineral charcoal. This substance consists of fragments of prosenchymatous and vasiform tissues in a carbonized state, somewhat flattened by pressure, and more or less impregnated with bituminous and mineral matters derived from the surrounding mass. It has resulted from the *subaërial* decay of vegetable matter; whilst the compact coal is the product of *subaqueous* putrefaction, modified by heat and exposure to air. The author proceeded (after describing the methods used by him in examining mineral charcoal and coal) to describe the tissues of Cryptogamous plants in the state of mineral charcoal. Among these he mentions *Lepidodendron* and *Ulodendron*, also disintegrated vascular bundles from the petioles of Ferns, the veins of Stigmarian leaves, and from some roots or stipes. He then describes tissues of Gymnospermous plants in the state of mineral charcoal; especially wood with discigerous fibres and also with scalariform tissue, such as that of *Stigmaria* and *Calamodendron*; and the author remarks that probably the so-called cycadeous tissue hitherto met with in the coal has belonged to *Sigillariæ*.

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The next chief heading of the paper has reference to structures preserved in the layers of compact coal, which constitutes a far larger proportion of the mass than the mineral charcoal does. The laminæ of pitch- or cherry-coal, says Dr. Dawson, when carefully traced over the surfaces of accumulation, are found to present the outline of flattened trunks. This is also true to a certain extent of the finer varieties of slate-coal; but the coarse coal appears to consist of extensive laminæ of disintegrated vegetable matter mixed with mud. When the coal (especially the more shaly varieties) is held obliquely under a strong light, in the manner recommended by Goeppert, the surfaces of the laminæ of coal present the forms of many well-known coal-plants, as *Sigillaria*, *Stigmaria*, *Poacites* (or *Nœggerathia*), *Lepidodendron*, *Ulodendron*, and rough bark, perhaps of Conifers. When the coal is traced upward into the roof-shales, we often find the laminæ of compact coal represented by flattened coaly trunks and leaves, now rendered distinct by being separated by clay.

The relation of erect trees to the mass of the coal, and the state of preservation in which the wood and bark of these trees occur,—the microscopic appearances of coal,—the abundance of cortical tissue in the coal, associated with remains of herbaceous plants, leaves, &c., are next treated of.

The author offers the following general conclusions:—

(1) With respect to the plants which have contributed the vegetable matter of the coal, these are principally the *Sigillariæ* and *Calamiteæ*, but especially the former.

(2) The woody matter of the axes of *Sigillariæ* and *Calamiteæ* and of coniferous trunks, as well as the scalariform tissues of the axes of the *Lepidodendreæ* and *Ulodendreæ*, and the woody and vascular bundles of ferns, appear principally in the state of mineral charcoal. The outer cortical envelope of these plants, together with such portions of their wood and of herbaceous plants and foliage as were submerged without subaërial decay, occur as compact coal of various degrees of purity, the cortical matter, owing to its greater resistance to aqueous infiltration, affording the purest coal. The relative amounts of all these substances found in the states of mineral charcoal and compact coal depend principally upon the greater or less prevalence of subaërial decay occasioned by greater or less dryness of the swampy flats on which the coal accumulated.

(3) The structure of the coal accords with the view that its materials were accumulated by growth without any driftage of materials. The *Sigillariæ* and *Calamiteæ*, tall and branchless, and clothed only with rigid linear leaves, formed dense groves and jungles, in which the stumps and fallen trunks of dead trees became resolved by decay into shells of bark and loose fragments of rotten wood which currents must have swept away, but which the most gentle inundations, or even heavy rains, could scatter in layers over the surface, where they gradually became imbedded in a mass of roots, fallen leaves, and herbaceous plants.

(4) The rate of accumulation of coal was very slow. The cli-

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mate of the period, in the northern temperate zone, was of such a character that the true conifers show rings of growth not larger, or much less distinct than those of many of their northern congeners*. The *Sigillariæ* and *Calamites* were not, as often supposed, succulent plants. The former had, it is true, a very thick cellular inner bark; but their dense woody axes, their thick and nearly imperishable outer bark, their scanty and rigid foliage would indicate no very rapid growth. In the case of *Sigillariæ*, the variations in the leaf-scars in different parts of the trunk, the intercalation of new ridges at the surface representing that of new woody wedges in the axis, the transverse marks left by the successive stages of upward growth, all indicate that at least several years must have been required for the growth of stems of moderate size. The enormous roots of these trees, and the conditions of the coal-swamps, must have exempted them from the danger of being overthrown by violence. They probably fell, in successive generations, from natural decay; and, making every allowance for other materials, we may safely assert that every foot of thickness of pure bituminous coal implies the quiet growth and fall of at least fifty generations of *Sigillariæ*, and therefore an undisturbed condition of forest-growth enduring through many centuries. Further, there is evidence that an immense amount of loose parenchymatous tissue, and even of wood, perished by decay; and we do not know to what extent even the most durable tissues may have disappeared in this way; so that in many coal-seams we may have only a very small part of the vegetable matter produced.

Lastly. The results stated in this paper refer to coal-beds of the middle coal-measures. A few facts which I have observed lead me to believe that in the thin seams of the lower coal-measures remains of *Nœggerathia* and *Lepidodendron* are more abundant than in those of the middle coal-measures†. In the upper coal-measures similar modifications may be expected. These differences have been to a certain extent ascertained by Goeppert for some of the coal-beds of Silesia, and by Lesquereux for those of Ohio; but the subject is deserving of further investigation, more especially by the means proposed in this paper, and which I hope, should time and opportunity permit, to apply to the seventy-six successive coal-beds of the South Joggins.

XLVIII. *Intelligence and Miscellaneous Articles.*

STEAM-SHIP PROPULSION.

THE following are the results of an anticipative calculation of the probable speed of the 'Great Eastern' in *nautical miles* an hour, with different amounts of indicated horse-power, when drawing

* Paper on Fossils from Nova Scotia, Proc. Geol. Soc. 1847.

† I may refer to my late paper on Devonian Plants from Canada for an example of a still older coal made up principally of remains of Lycopodiaceous plants of the genus *Psilophyton*.

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twenty-eight feet of water:—

Indicated horse-power.	Probable speed, knots.
7000	13
8000	13·6
9000	14·1
10000	14·6
11000	15·1
12000	15·6
13000	16

March 21, 1859. R.

NOTE ON THE POLARIZATION OF THE LIGHT OF COMETS. BY SIR DAVID BREWSTER.

Although there can be no doubt as to the accuracy of the observations of M. Arago on the indications of polarization discovered by him in the light of the comets from 1819 to 1835, there is nevertheless nothing impossible in the supposition that the light may have been polarized after arriving in the terrestrial atmosphere. In fact, when we consider that light is polarized by refraction in passing through the coats of the eye, that it is polarized by refraction at the four or six surfaces of the object-glasses of an astronomical telescope, and also in passing through the surfaces of its eyepiece, and, lastly, that the light of celestial bodies undergoes a slight polarization by the refraction of the atmosphere, we are compelled to admit that the problem of the existence of polarized light in the light of comets is not solved.

I am not aware that those who have observed traces of polarization in the light of comets have noted the direction of the plane in which it has been polarized; nevertheless without some such observation we cannot discover its cause. If the light be polarized in a plane passing through the sun, the comet, and the eye, we must infer that it is polarized by the *reflexion* of the light coming from the sun; if it be polarized in an opposite plane, the polarization may be due to the *refraction* of the atmosphere. If it be polarized *quaquaversus*, this may be due to three causes; namely, to refraction by the surfaces of the object-glasses and eyepiece, to an imperfection in the annealing of the glass of which the lenses are formed, or to the fact of one or more of the lenses being pinched in their cell. Supposing it to be an effect of the first of these causes, the openings of the object-glasses and eyepiece should be reduced to a central band, which would eliminate the light polarized in an opposite plane, and leave that which it polarized in a plane perpendicular to the direction. By turning the telescope or the lenses, the direction of the polarization would be changed.

If the polarization be produced by a defect in the annealing of the glass of which the lenses are made, as appears to be the case in one of Amici's telescopes mentioned by M. Govi, the existence of this imperfection will be rendered evident by exposing the lenses to polarized light.

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If the polarization observed be due to the reflexion of the rays of the sun by the comet or its envelopes, small stars will be seen more distinctly through it when the polarized light is extinguished by the application of a Nicol's prism.

Whilst I was investigating the polarization of the atmosphere, I observed the remarkable fact, that when objects situated far off in the open country are rendered indistinct by the interposition of a light mist, a part of their distinctness may be restored by viewing them through a Nicol's prism, which extinguishes all the light polarized by the mist in a plane passing through the sun, the object, and the eye of the observer. The objects thus rendered more distinct and visible were seen through that portion of the mist in which the polarization of the light reflected by them was at its maximum. This method of rendering visible objects rendered indistinct by fogs or mists may, it appears to me, receive important applications in military and naval operations.—*Comptes Rendus*, February 21, 1859, p. 384.

NEW APPARATUS FOR OBSERVING ATMOSPHERIC ELECTRICITY.

BY PROF. W. THOMSON.

I have had an apparatus for atmospheric electricity put up on the roof of my lecture room, and got a good trial of it yesterday, which proved most satisfactory. It consists of a hollow conductor supported by a glass rod attached to its own roof, with an internal atmosphere kept dry by sulphuric acid: the lower end of the glass rod is attached to the top of an iron bar, by which the hollow conductor is held about two feet above the inclined roof of the building. A can, open at the top, slides up and down on the iron bar which passes through a hole in the centre of its bottom, and, being supported by a tube with pulleys, &c. below, can easily be raised or lowered at pleasure. A wire attached to the insulated conductor passes through a wide hole in the bottom of the can, and is held by a suitable insulated support inside the building, so that it may be led away to an electrometer below. To make an observation, the wire is connected with the earth, while the can is up, and envelopes the conductor—its position when the instrument is not in use. The earth connexion is then broken, and the can is drawn down about eighteen inches. Immediately, the electrometer shows a large effect (from 5 to 15 degrees on my divided ring electrometer, in the state it chanced to be in, requiring more than 100 degrees of torsion to bring it back to zero, in the few observations I made). When the surface of the earth is, as usual when the sky is cloudless, negative, the electrometer shows positive electricity. But when a negative cloud (natural, or of smoke) passes over, the indication is negative. The insulation is so good that the changes may be observed for a quarter of an hour or more; and when the can is put up, the electrometer comes sensibly to zero again, showing scarcely any sensible change when the earth connexion is made, before making a new start.—*Extract of a letter to Dr. Joule in the Proceedings of the Manchester Philosophical Society.*

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THE

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PHILOSOPHICAL MAGAZINE

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JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MAY 1859.

XLIX. *On the Semidiurnal Oscillation of the Barometer.*

By W. S. JEVONS, *Assayer, Sydney Branch of the Royal Mint**.

1. TO explain the remarkable double oscillation of the barometer which occurs during each day in most parts of the world, a theory has been proposed by the eminent meteorologists Dove and Sabine, and has, I think, been favourably received by the scientific world, which I yet venture to call in question. It is concisely stated in Sabine's Notes to 'Cosmos†,' and is somewhat as follows:—In all countries which are not far removed from large surfaces of water, the barometer on an average stands highest about 9 A.M. and 9 P.M., and it stands lowest at the nearly intermediate hours of 2 or 3 P.M., and 4 or 5 A.M. (sunrise). This phænomenon, being of daily occurrence, can only depend ultimately on the sun's heat and the rotation of the earth; the intermediate causes yet admit of debate. Now as the temperature of the air only undergoes a single oscillation during the day, it could only, and in some parts of the world does only occasion a single minimum of the barometer during the day, and a single maximum at sunrise; it is supposed, in fact, that this is always the effect produced by the agency of temperature. But the aqueous vapour contained in the air also undergoes a daily variation; its elastic force is greatest in the daytime and least at night, and this varying force must equally affect the barometrical column. Now the two undulations thus continually produced in an independent manner are superimposed so as to disguise each other, and the result is the semidiurnal oscillation of pressure under consideration. It is the great deficiency of aqueous vapour at sunrise which causes a

* Communicated by the Author.

† Vol. i. p. xcix.

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minimum of the barometer at that period of the day, although the low temperature then existing would have an opposite effect; it is the great increase of vapour which raises the barometer to a maximum at 9 A.M.; but the increasing temperature of the air then becomes predominant in its effects, and the afternoon minimum of pressure is the consequence. The second maximum at 9 P.M. is to be ascribed to the subsequent rapid decrease of the temperature.

2. This theory is supported by many elaborate and ingenious researches, by Dove and Sabine, into the daily barometrical curve peculiar to various situations. In the interior of Russia (and apparently in Russia only) the curve is of most simple character, the barometer having a single maximum at sunrise and a single minimum in the afternoon: this peculiarity is supposed to be owing to the comparative absence of aqueous vapour in such far inland places, the temperature there becoming the entirely predominant agent.

Colonel Sabine takes the opportunity, when speaking of this theory, to eulogise "the progress which is made in the physical sciences by the aid of *mean numerical values*," and the "new aspect which this beautiful branch of investigation has assumed by the *separation of the pressures of the aqueous and gaseous portions of the atmosphere*." Now, while fully admiring the method of investigation by *mean results*, I must object to the almost exclusive employment of it which now seems usual in meteorology and some other sciences; and I proceed to show that in the case of the barometrical oscillation it has led to serious misconceptions, which a little close inquiry into the known nature and the simplest mechanical conditions of the atmosphere must have prevented.

3. The error consists in the whole practice of *separating the aqueous and gaseous pressures of the atmosphere*, and is in origin perhaps an *error of terms*. It is said, indeed, and proved, that the pressure of aqueous vapour is independent of the pressure of the air. Thus if water be introduced into a perfect vacuum, a definite quantity will instantly rise from it and exert a definite pressure on the sides of the confining vessel; for instance, at the temperature of 60° F., the pressure exerted will be nearly equal to that of a column of mercury half an inch deep. But if the space into which we introduce the water, instead of being previously vacuous, contain air of any given density and elastic force, the quantity of aqueous vapour emitted will still be the same (or very nearly), and the pressure of this vapour, which is equal to that of half an inch of mercury, will be exerted against the confining vessel, in addition to the previous pressure of the air.

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Thus far the aqueous vapour appears to be quite independent of the air; but it should be remarked that whereas water, when introduced into a vacuum, instantly exerts its elastic pressure on all sides, and instantly diffuses itself through the whole space, it does not, in the presence of air, add its independent pressure with equal instantaneity, but, on the contrary, only *slowly diffuses itself*, or makes its way among the particles of air; and the more condensed the air within the vessel, the more slowly will the aqueous vapour thus diffuse itself.

4. Now a confined vessel full of air does not truthfully represent the atmosphere, which has no upper confining surface. The pressure of the atmosphere is only the effect of gravity; and at any given place the elastic force which it exhibits is only due to the superincumbent weight of air. If the earth's atmosphere were suddenly removed, the water on its surface would evaporate with excessive rapidity, and we should soon have a purely aqueous atmosphere, of which the pressure would be closely limited by the existing temperature. But now, I ask, if the atmosphere were supposed to be suddenly rendered perfectly free from aqueous vapour, would the evaporation from the water on the earth's surface be so rapid as almost instantly to produce an aqueous atmosphere coextensive with the gaseous one? And, secondly, would the pressure of the gaseous atmosphere be instantly increased by the amount of the elastic force of vapour due to the temperature at the surface of the earth? To both these questions the answer must be—*No, only very gradually.* It need hardly be said that aqueous vapour is impeded in its motion by the presence of air, and that the one only diffuses slowly through the other. It would be a very long time before the atmosphere could become completely saturated, just as a certain length of time is required for a confined body of air to become so.

But the pressure also of the atmosphere could only increase in proportion as the aqueous vapour became diffused through it, because gravity is the only cause of such pressure, and the mere fact of water lying on the surface of the earth cannot increase the weight of what lies above. *The weight of the atmosphere cannot be greater than the weight of its components,* of which aqueous vapour is one. The lowest strata of the atmosphere will, indeed, soon become saturated with aqueous vapour rising from the moist surface of the earth; that is to say, the hygrometer will there indicate aqueous vapour of the greatest elastic force possible at the existing temperature; but the real pressure of the atmosphere will only be increased by the actual weight of water raised into it by evaporation. Although, within four feet or within a mile of the earth's surface, the hygrometer indicates an aqueous pressure

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equal to half an inch of mercury, the weight or barometric pressure of the atmosphere will not be increased by this amount until the whole column of the atmosphere is saturated, or, more correctly speaking, until the independent aqueous atmosphere is fully established by slow diffusion through the gaseous one.

5. A confined body of air evidently differs in its mechanical conditions from the atmosphere, in the fact that the aqueous vapour reacts against the upper confining surface of the former, and is thus enabled instantaneously to exert a similar pressure on all sides; but the atmosphere is unconfined except by its own weight, and affords no fixed fulcrum. If air were impermeable to aqueous vapour, none could possibly rise except at a temperature exceeding the boiling-point, when the rising vapour would become able to lift the atmosphere in mass. But the air is permeable; and at a temperature of 60°, for instance, water, although totally unable to lift the atmosphere by its elastic force, projects aqueous vapour into it and causes it gradually to ascend with a force equal to half an inch depth of mercury. The elastic force of the aqueous vapour is therefore exerted against *the obstructive power of the air*. Ultimately, when the permeation of the one through the other is complete, the aqueous vapour will exert pressure only against itself, and will be so far independent of the air. The atmosphere will then, but not till then, have increased in total weight or barometric pressure, by the amount of the elastic force of vapour indicated by the hygrometer at the surface of the earth.

6. The above rather tedious but incontrovertible reasoning amounts to a *reductio ad absurdum* of the hypothesis that aqueous vapour adds its independent elastic force to the pressure of the gaseous atmosphere, a hypothesis which can only have arisen from a confusion of terms. The actual weight of the atmosphere is what occasions its pressure as measured by the barometer. If we wish therefore to separate the aqueous and gaseous pressures, we must separate (not the elastic force of vapour at the surface, but what is very different) *the actual weight of aqueous vapour contained in the whole column of the atmosphere from the united actual weight of nitrogen, oxygen, and carbonic acid.*

7. This being clearly understood and allowed, it is almost needless to say that the observation of a hygrometer at the surface of the earth cannot inform us how much aqueous vapour is diffused through the lighter strata of air. The balloon observations of the British Association sufficiently prove this; for in the ascent of November 10, 1852, Mr. Welsh determined that within an elevation of 22,930 feet the fluctuations of the humidity were numerous, "there having been no fewer than four or perhaps five different strata of vapour." It is further almost certain, as

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it is indeed almost self-evident, that, within a very moderate elevation above the surface of the earth, the humidity as well as the temperature of the air, cease to be affected by the daily variation. To argue, then, upon the variations of humidity at the surface of the earth as if they extended equally throughout the atmosphere, as is done in the prevalent theory, is certainly erroneous.

8. But it remains to be considered how far the daily variation of humidity, such as it really exists, is capable of affecting the barometer. This we may easily estimate, not by using such a complex mode of measurement as the hygrometer, but by simply observing what weight of water enters or quits the atmosphere during the various periods of the day. For it is evident that if a cubic foot of water, instead of resting on the surface of the earth, evaporate and form a part of the atmosphere, the weight of the latter will be increased by the precise weight of the cubic foot of water. Similarly, if a stratum of water a foot deep evaporate all over the world, the pressure of the atmosphere in every place will increase by an amount represented by one foot depth of water, which is equal to a column of mercury 88 hundredths of an inch in depth, since mercury is 13·6 times as dense as water. In short, the evaporation of a foot of water will cause the barometer to rise about nine-tenths of an inch. The precipitation of a foot depth of rain, supposing it to extend equally over all parts of the globe, must occasion a corresponding fall of the barometer.

Now applying these considerations to one particular subject, namely the semidiurnal variation of the barometer, I argue that the state of the hygrometer is a matter of perfect indifference, and that the evaporation of water by day and its deposition as dew at night, or as rain at occasional times, are the only modes in which the barometer will be appreciably affected through the agency of humidity*. Thus, in a place where the evaporation is 30 inches in the year, or ·082 inch in each day on the average, the barometer cannot be affected thereby to a greater extent on an average during each day than ·006 inch (or ·082÷13·6); and if, with Dalton, we estimate the yearly depth of dew at 5 inches, its average daily effect on the barometer will be only ·001 inch. These estimations perhaps apply roughly to England, and are the total amounts which we have at our disposal to explain a daily oscillation of the barometer, there amounting to about ·030 inch, even supposing that evaporation takes place wholly in such a manner as to assist the prevalent theory.

* Aqueous vapour, while it so remains, expands and contracts by heat and cold like any other gas, and may therefore have a trifling effect upon the barometer in the same manner as the gaseous atmosphere.

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9. But, to inquire more precisely what effect the daily variation of humidity will have upon the barometer, I refer to the following Tables containing the average results of twenty-six series of hourly observations undertaken by myself in Sydney, at monthly intervals during the years 1855, 1856, and 1857.

Table showing the daily variation of the Meteorological Elements at Sydney, New South Wales.

Hour.		Pressure.	Temperature of air.	Elastic force of vapour.	Evaporation.
		Departure from mean in inches.	Departure from mean in degrees Fahrenheit.	Departure from mean in inches.	Amount during previous hour in inches.
A.M.	6	+·010	− 6·4	−·030	·000
	7	+·023	− 4·8	−·020	·003
	8	+·032	− 2·1	−·007	·007
	9	+·035	+ 1·3	+·007	·011
	10	+·029	+ 4·6	+·022	·019
	11	+·015	+ 7·3	+·029	·023
Noon.		−·007	+ 9·3	+·038	·026
P.M.	1	−·024	+10·1	+·028	·029
	2	−·036	+ 9·6	+·022	·029
	3	−·041	+ 8·6	+·022	·021
	4	−·045	+ 7·4	+·020	·014
	5	−·034	+ 4·3	+·022	·010
	6	−·019	+ 1·3	+·013	·006
	7	−·005	− 0·5	+·002	·003
	8	+·007	− 1·3	−·001	·002
	9	+·018	− 2·7	+·001	·001
	10	+·021	− 4·0	−·008	·001
	11	+·017	− 4·7	−·011	·001
	12	+·013	− 5·1	−·015	·000
A.M.	1	+·006	− 5·8	−·022	·001
	2	−·003	− 6·4	−·024	·000
	3	−·008	− 6 5	−·026	·000
	4	−·007	− 6·9	−·027	·000
	5	−·001	− 7·1	−·029	·000

The above results nearly correspond with those derived from hourly observations uninterruptedly carried on for eight years at the Royal Observatory of Hobart Town, Van Diemen's Land, the evaporation, however, not being there determined.

It is seen that in Sydney the maxima of the barometer occur at 9 A.M. and 10 P.M., and the minima at 4 P.M. and 3 A.M. These changes of the barometrical pressure, as well as the corresponding changes of the various elements from which an explanation might be sought, are shown below.

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Changes of the Meteorological Elements during the daily barometric periods.

Barometric period.	Barometer in inches.	Temperature of air in degrees Fahrenheit.	Elastic force of vapour in inches.	Evaporation in inches.	Equivalent pressure in inches of mercury.
3 A.M. to 9 A.M.	+·043	+ 7·8	+·033	·021	·0015
9 A.M. to 4 P.M.	−·080	+ 6·1	+·013	·161	·0118
4 P.M. to 10 P.M.	+·066	−11·4	−·028	·023	·0017
10 P.M. to 3 A.M.	−·029	− 2·5	−·018	·002*	·0003

10. We have here a clear comparison of the theory of the semidiurnal oscillation as generally received, with that condition of it to which my arguments would reduce it. If it were true, as it probably is not, that the temperature of the air acts *inversely*, and the elastic force of vapour at the surface of the earth acts *directly* upon the barometer, the motions of the latter would be to some extent at least explained. But it is the numbers in the last column of the Table which truly express the whole effect of the daily variation of humidity upon the barometer. They represent the height of a column of mercury, of which the pressure is equivalent to that of the water which evaporates during each period of the day. It is evident that *not only are these numbers quite insignificant in amount, but that they also bear no apparent relation whatever to the barometric oscillations.*

The humidity of the air is, as a general rule, always increasing by evaporation from the surface of the earth, and the barometer therefore has a tendency to be continually rising. But by far the largest amount of evaporation is observed to take place between 9 A.M. and 4 P.M., during which time the barometer falls as much as eight-hundredths of an inch. Neither can the upward motion of the barometer from 3 A.M. to 9 A.M. and from 4 P.M. to 10 P.M. be explained by an increase of humidity, because the real amount of this increase is so minute. It is true, indeed, that during the night dew is often deposited, the tendency of which would be to produce a minimum of the barometer; but the average amount of dew, I consider, is altogether inadequate to this effect in England or any other country.

11. The prevalent theory, that the semidiurnal barometric oscillations are partially owing to the variations of humidity, seems to have been derived from the practice of separating the

* Dew was often detected during this period to the extent of a few thousandths of an inch; but as the opposite process of evaporation sometimes proceeded slowly throughout the night, the dew does not appear in the average result.

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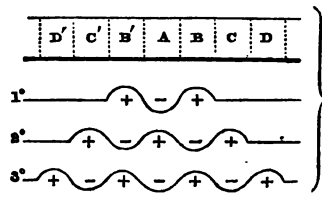
amount of the elastic force of vapour, as indicated by a hygrometer at the surface of the earth, from the total pressure of the atmosphere, and considering the remainder as the gaseous pressure. I have attempted to prove that this practice is erroneous, and *can have no possible use or meaning*; so that the theory in question will thus be without foundation. But even if we adopt the true mode of estimating the barometric effects of the variation of humidity, I have brought some results to show that no explanation is obtained of the oscillations of the barometer.

12. It will not have escaped my readers that, as a further consequence of my arguments, the weight of the atmosphere must be diminished by the weight of water falling from it as rain, and that the barometer must be thereby affected in some place or other. The precipitation of a foot depth of rain over the whole surface of the earth must cause a uniform fall of the barometer to the extent of ·088 inch. But the varied conditions under which evaporation and the fall of rain take place, as well as the many great and complex effects which the removal of incomprehensibly vast bodies of water from place to place must occasion, form part of the general *mechanical problem of the atmosphere* in which meteorology really consists. I will only notice that, in a previous paper "On the Forms of Clouds" (Phil. Mag. April 1858), I have shown this same *motive force*, viz. the diminution of weight and pressure produced by the separation of cloud-particles and rain, to be the probable cause of those motions between masses of air which constitute the cirrus, the cumulostratus, and even the thunder-cloud.

13. *Probable explanation of the semidiurnal oscillation of the barometer**.—The doubly-pointed curve which the barometer describes during the day, simply indicates a double undulation occasioned in the atmosphere by the disturbance of the sun's rays. This may be explained as follows:—Let fig. 1 represent an atmosphere reposing on a horizontal surface, divided into imaginary columns. If the centre column A be subjected to the action of the sun's rays for any given period, say six hours, the air will increase in volume and will overflow upon the adjoining columns B and B′. The barometric pressure will therefore diminish at the base of A and increase at the bases of B and B′, so that undulations will exist as shown by the line 1°; and

Fig. 1.



* Being without means of reference, I am unaware whether this theory is entirely new; I have not myself seen it anywhere stated.

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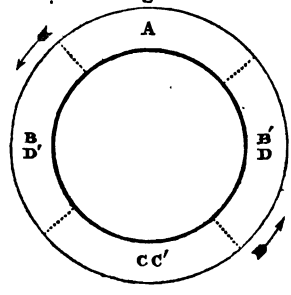
like other undulations, such as waves on the surface of water, will spread to an indefinite distance on each side. But suppose that, before they have passed beyond B and B′, the action of the sun upon A ceases, and terrestrial radiation produces a counteraction. The column A contracts; and the columns B and B′, possessing superior height and weight, will force back a portion of air upon A, while other portions overflow upon C and C′. There will soon be a minimum of pressure at the bases of B and B′, and a maximum at the bases of A, C, and C′, as shown by the line 2°. If the sun again act upon A, and even, indeed, if it do not again interfere, the undulatory motion will proceed, and at the next step we shall have the condition of things shown by 3°, and so on indefinitely.

14. Upon several mechanical conditions will depend the rate at which the undulations will spread. It is plain that if the action of the sun recurs at intervals exactly equal to those occupied by the passage of each undulation, its effect will be to increase and maintain the undulatory motion, which otherwise would die out by friction. From this cause, too, the undulations will continually decrease in height as they spread. If the intervals are not equal, the sun will soon counteract the undulatory motion.

15. Next, instead of supposing the sun to act only on A, let it travel along, say over B, C, D, &c., and at such a rate as exactly to keep pace with the undulations. After acting for six hours upon A, and producing a minimum there as in 1°, the sun will begin to act upon B just as a maximum of pressure has occurred, and its effect will be greatly to increase the overflow of air upon A and C, and assist the production of a minimum in B and of maxima in A and C. It will further reach C just as the maximum has there occurred, its effect being precisely similar to that upon A and B, and so on indefinitely. The condition, however, still is that the sun moves exactly at the same rate as the undulations; otherwise it will neutralize its own effect.

Fig. 2.



16. To apply this reasoning to the actual condition of the atmosphere, we have only to suppose the stratum of atmosphere extended round the earth, of which the section is a circle, as in fig. 2, and the sun to revolve round the earth, as it apparently does in a period of twenty-four hours, in the direction A, B, C, D. Its action upon A will produce a minimum of pressure there

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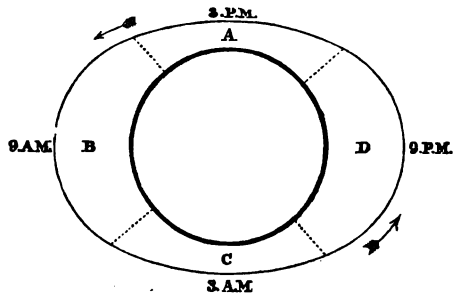
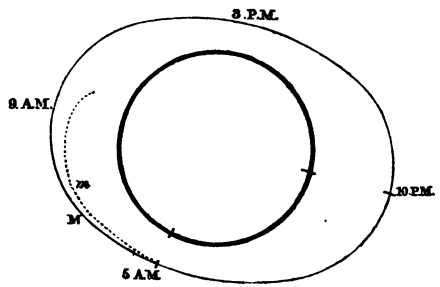
and maxima at B and B′; and, assuming the undulations thus produced to travel at such a rate as would carry them round the globe in twenty-four hours, the sun will then commence to act upon B at the moment at which its maximum of pressure has happened. The succeeding minimum at B will therefore be increased, and maxima will be produced at A and C′. During the same time also the maximum pressure at B′ will occasion an overflow upon A and C′, thus assisting the direct action of the sun. The same undulatory motion will ensue as the sun passes round to C and D; and it is evident, as a simple mathematical consequence of the supposed conditions, that at each point of the circle maxima and minima of barometric pressure will succeed each other at intervals of six hours. This is represented in fig. 3, in which the ellipse is the shape assumed by the atmosphere, and is supposed to rotate round the earth in the same period as the sun.

Fig. 3.



17. If, lastly, we suppose that an atmospheric undulation produced by the sun's action does not travel at the same rate as the sun, the question becomes more complex, and the curve produced loses its symmetry. The sun probably travels a little the faster, so as perpetually to overtake the preceding undulation before it is quite accomplished. The effect of this may perhaps be best understood from fig. 4, in which is shown the partial interference of the maximum (M) immediately produced by the sun, with the maximum (*m*) occasioned by the sun's action twelve hours previously. The further prosecution of this problem belongs rather to the mathematician than the meteorologist; but it is not hard to see from this figure why the night minimum is in most places deferred till sunrise (5 A.M.), and why it is less strongly marked than that occasioned directly by the sun's rays.

Fig. 4.



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18. On this theory, then, the semidiurnal oscillations are supposed to be occasioned by two vast waves perpetually moving round the earth, almost like the tides of the ocean. Air of of course would not be carried round by these waves, but a general swaying to and fro of the upper parts of the atmosphere would be the only actual motion occasioned. The waves originate in the sun throwing the air on each side of him; hence there must be a constant tendency to an easterly wind before noon, and a westerly wind in the afternoon.

These daily barometric waves are of greatest height and regularity in the equatorial regions, and are not appreciable in the polar regions*. The varied shape of oceans, continents, and even mountain-chains must produce irregularities, and the change of seasons must occasion modifications of the wave-curve, to discover and explain which will afford employment to numberless students of meteorological science for many years to come. Eventually we may hope that an endless variety of facts, only apparently capricious, will be made to harmonize together under a simple mechanico-mathematical theory, and the science of the atmosphere will be raised to a new position.

Sydney, New South Wales,
January 10, 1859.

L. *On the Coloured Houppes or Sectors of* Haidinger. *By* Sir David Brewster, *K.H., D.C.L., F.R.S., and Foreign Associate of the Institute of France*†.

M. HAIDINGER'S fine discovery of the coloured houppes or sectors which are visible in polarized light, and indicate its plane of polarization, is one of great interest both in its optical and physiological relations. Having always believed that they were produced by a peculiar structure in the eye, placed between the vitreous humour and the sclerotic coat, I could not adopt the ingenious explanation of them given by M. Jamin, and I have therefore been induced to examine them with some care.

In order to discover their cause, we must ascertain their size, their form, their colour, and the intensity of their light.

1. In reference to their size, M. Haidinger states that each has "an apparent diameter of nearly *two* degrees," that is, *four*

* It is probable that, during the passage of the sun, a certain overflow of air would take place from the equatorial and temperate upon the polar regions, followed by a corresponding reflux. An inversion of the barometric curve would hence result in the polar regions, such as is thought to occur. (See 'Cosmos,' Sabine's translation, vol. i. p. 309.)

† Communicated by the Author.

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degrees for the diameter of both. I have measured them with great care, and have received measurements from others, and I find the greatest diameter of the two to be $4\frac{1}{2}$ degrees.

2. The houppes or *coloured sectors* have a different appearance to different persons. The Abbé Moigno describes them, and M. Haidinger has drawn them, as resembling a bundle of pale-yellow twigs bound tightly together at their middle, and having on each side of the narrowest part of the bundle two small masses of *violet* or *blue* light*. Afterwards, however, the Abbé made a most important observation, described and drawn by M. Haidinger†. He observed the blue masses or sectors crossing the middle part of the yellow bundle now represented by this separation, as consisting of two yellow circular spaces.

In the numerous observations which I have made, the yellow sectors have the appearance as drawn by M. Haidinger, that is, there is a certain breadth of yellow light in the narrow part of the bundle of yellow twigs; but they have this appearance *only when they have a vertical position, that is, when they are perpendicular to a line joining the eyes. At right angles to this position the blue sectors or masses encroach upon the yellow, and occupy the middle of the yellow bundle.* When the head is turned round, the yellow bundle with its middle part turns round also, and is always perpendicular to the line joining the eyes, while the blue masses or sectors united are always in that line. Reckoning from the middle point of the yellow sectors, the angle formed by each of them does not exceed 65 degrees, so that the angle of the blue sectors must be 115 degrees each.

3. The colour of the houppes or sectors is a very *faint yellow*, and a *pale blue* fully as bright as the yellow.

According to M. Jamin, the yellow sectors are nothing more than portions of the polarized beam which are refracted more copiously by the cornea and crystalline when the refraction is made in or near a plane perpendicular to the plane of primitive polarization, than when it is refracted in or near that plane.

"The refracted light," he says, "will therefore exhibit in the plane of polarization *two obscure sectors* (*aigrettes*) united at the centre by their summits, widening towards the circumference, and *two brilliant sectors of the same form* in the perpendicular plane.

The colour of the light thus refracted must be *slightly yellow*, as I have long ago proved‡; and M. Jamin finds in this yellow light the cause of the yellow sectors, while he regards the two

* *Répertoire d'Optique Moderne*, p. 1326.

† Poggendorff's *Annalen*, vol. lxvii. p. 435; and *Répertoire*, &c., p. 1362.

‡ Phil. Trans. 1815, p. 152. Prop. XXV.

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obscure sectors as made blue by contrast. In order to test this explanation, we must mention that M. Zokalski, an oculist in Paris, found four persons from whose eyes the crystalline lens had been extracted, and who nevertheless saw the phænomenon of the coloured sectors. Hence it follows that, as the inner surface of the cornea has very little refraction, from its being in contact with the aqueous humour, the sectors of M. Haidinger must be produced, on M. Jamin's hypothesis, almost solely by the anterior surface of that membrane.

Ingenious as this explanation is, it is liable to the following objections:—

1. The magnitude of the sectors does not vary, as it ought to do, with the aperture of the pupil, and with the area of the polarized beam.

2. The yellow sectors ought in every position of the eye and of the polarizing prism to have the same form in all sound corneas: but in the Abbé Moigno's eye, the obscure sectors stretch across the centre and separate the yellow ones; and in my observations the phænomenon varies, as already described, with the motion of the head or of the polarizer.

3. The *yellow* colour of the sectors is not that which is produced by the refraction of polarized light. Their colour is the *yellow* of the *second* order of Newton's scale, whereas the yellow produced by refraction, even at the maximum polarizing angle, has an entirely different hue. But even if the colours were the same, a single refraction, or even several at such a small angle of incidence as that at which the rays are refracted by the cornea, could not produce a visible tint.

4. The *blue* sectors being, according to Jamin, brilliant, and certainly as bright as the *yellow* ones, it is impossible that their colour can be produced by contrast. I have made many experiments with yellow and obscure sectors of various relative intensities, and I cannot perceive anything resembling the *blue sectors* in question.

5. If the sectors are produced by refraction, the angular magnitude of the *yellow* ones ought to be greater than 65 degrees, and ought even to exceed that of the obscure ones. This result I have put to the test of direct experiment, by transmitting polarized light through various combinations of very small and very convex surfaces of glass. Even when these surfaces are numerous, the luminous sectors are greatly larger than the obscure ones.

6. If the yellow sectors are produced by the refraction only of the cornea, their feeble tint should be increased by placing before the eye a number of concavo-convex surfaces like those of the cornea. I have made this experiment, but no change is produced either in the form or intensity of the sectors.

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As this question is entirely an experimental one, I have sought for an *experimentum crucis* independent of the preceding considerations. The two following experiments appear to me to have this character:—

1. If in place of transmitting the polarized light through the whole aperture of the pupil, we look through a small pin-hole the fiftieth or sixtieth of an inch in diameter, the coloured sectors are seen of the same size and form, but only fainter, as with the pupil when its aperture is a maximum.

2. If we look through the narrowest slit that admits a sufficient quantity of light, and give it a motion of rotation in front of the cornea, so that the coloured sectors may be examined when produced by light incident in every azimuth, we shall find that the sectors neither change their position nor their form, and consequently cannot be produced by the refractions which are made by the cornea in every azimuth of polarization.

If these views be correct, it follows that the structure which produces the coloured sectors must exist at the end of the optical axis of the eye, and in the retina between the vitreous humour and the sclerotic coat. The existence of such a structure can be determined only by anatomists; and hence the true cause of the coloured sectors cannot be discovered till the structure of the retina is better known. The drawings of this membrane by Brucke and Kölliker, and of the *foramen centrale* by Soemmering, exhibit no combination of polarizing and refracting surfaces capable of explaining the optical phænomenon; but in the more recent dissections of Mr. Nunneley of Leeds, published a few months ago in his 'Treatise on the Organs of Vision,' there are obvious traces of the required structure.

I have long ago proved by unquestionable experiments, that there is a part of the retina, corresponding with the sectors, and exactly of the same angular magnitude, which becomes sensible to light sooner than the rest of the retina, and shows itself in a dark reddish spot $4\frac{1}{2}$ degrees in diameter. This property must be the result of structure; and if we suppose the spot to be covered with a polarizing film, or to have the structure of one, all that we require to explain the phænomenon of the sectors is a few refractions, accompanied with polarization, like those which take place at the cornea, or with a number of convex or concave surfaces. Now it is a remarkable fact, that in Mr. Nunneley's drawing of the *foramen centrale* of the retina there is the appearance of such a structure*; and as this membrane consists of *eight* layers, including structures of different forms, it is probable that the refractions which take place at each, small though they must be, may be sufficient to produce the optical phænomenon.

* Plate 1. fig. 19.

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LI. *On the Thickness of the Crust of the Earth. By the Venerable* JOHN HENRY PRATT, *M.A., Archdeacon of Calcutta.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE question whether the earth's crust is of comparatively small or great thickness is one of considerable interest to geologists, more especially because of its bearing upon the theory of volcanoes. The only mathematicians who have taken up the subject as a branch of physics are, as far as I am aware, Messrs. Hopkins, Hennessy, and Haughton*. The result of Mr. Hopkins's investigations is, that the thickness is very considerable, amounting to as much as 800 or 1000 miles. Mr. Hennessy comes to the conclusion that the least possible thickness is 18 miles, and the greatest 600 miles. Mr. Haughton makes the thickness less than 768 miles, but adds that, in fact, "the subject would appear to be excluded from the domain of positive science, and to possess an interest for the mathematician only."

2. My present object is to point out what appears to me to be a fallacy in the last gentleman's reasoning, and also an inadmissible assumption in that of the second. Before doing this I will trace the evidence upon which Mr. Hopkins's conclusion rests. (The notation is changed, as it is not the same in the papers I quote in this communication.)

Mr. Hopkins has deduced the following formula,

$$\frac{P'-P}{P}=1-\frac{\epsilon}{\epsilon}-\frac{\int_a^a \rho'\frac{d\,.\,a'^5(\epsilon'-\epsilon)}{da'}da'}{2\epsilon a^3\int_0^a \rho' a'da'+\epsilon\int_a^a \rho'\frac{d\,.\,a'^5}{da'}da'};$$

where P is the precession of a homogeneous spheroid of ellipticity ϵ; P' of a heterogeneous shell, composed of nearly spherical strata, the outer and inner ellipticities being ϵ and ϵ, a and a the mean radii of the bounding surfaces; $a'\epsilon'\rho'$ the mean radius, ellipticity, and density of any stratum of the general mass, solid or fluid.

If we assume that the strata decrease in ellipticity in passing downwards (which is the result of the usual theory of the earth's figure), $\epsilon'-\epsilon$ is never negative, and the fraction on the right hand is never negative, and is never so large as unity. Let it $=\beta$. Calculation makes $P=57''$ nearly; observation makes

* Mr. Hopkins in Phil. Trans. for 1839, 1840, 1842; Mr. Hennessy in Phil. Trans. for 1851; Mr. Haughton in the Transactions of the Royal Irish Academy for 1852. It is only about a week ago that I first saw the last of these papers.

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$P' = 50''{\cdot}1$. Hence the above formula gives

$$\frac{\epsilon}{\epsilon} = \frac{7}{8} - \beta, \text{ or } \epsilon \text{ less than } \frac{7}{8}\epsilon.$$

Hence, because the ellipticity decreases in descending, the thickness must be greater than would correspond with an ellipticity of the inner surface of the shell $= \frac{7}{8}$ ellipticity of the outer surface.

If solidification took place solely from pressure, the surfaces of equal density would be surfaces of equal solidity. From the ordinary theory of the figure of the earth, considered as consisting of strata (whether fluid or not) which follow the fluid law and the law of density $\frac{\sin qa}{a}$, Mr. Hopkins shows that if the ratio of the mean density to the superficial density $= 2{\cdot}4225$, the thickness of the crust must be one-fourth of the radius, if the ellipticity of the inner surface $= \frac{7}{8}\epsilon$; if the ratio of densities is a little larger than 3, the thickness is one-fifth of the radius. But the ratio of densities is generally regarded as less than the smaller of the above ratios. Hence we shall not be over the mark in stating that the crust, under the hypothesis that the solidification is produced by pressure alone, cannot be less than one-fifth or one-fourth of the radius in thickness.

But solidification depends upon temperature as well as pressure, and Mr. Hopkins shows (Phil. Trans. 1842) that the isothermal surfaces increase in ellipticity in passing downwards. Now the ellipticities of the surfaces of equal degrees of solidity must, under the double action of temperature and pressure, lie between those of temperature and pressure considered alone. Hence the surface of equal solidity passing through any point will be more elliptic than the surface of equal pressure or density passing through that point; and therefore the inner surface of a solid shell, of which the ellipticity is $\frac{7}{8}\epsilon$, will be lower down in the mass than a stratum of equal density of that same ellipticity; and the shell will be thicker than if pressure alone cause solidification. Also since β is not negative, the shell for that reason will be thicker. The thickness, therefore, of 800 or 1000 miles is by no means too large a limit.

3. Mr. Hennessy's greatest thickness is 600 miles; and his conclusion is that the thickness may be so small as 18 miles. But his whole calculation proceeds upon the assumption that the shell is sufficiently rigid to resist, without change of form, the pressure upon its inner surface which arises from its not continuing to be a surface of fluid-equilibrium. He supposes also that the inner nucleus shrinks in cooling more than its solid envelope, which is hardly allowable. Under these circumstances,

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his conclusion can hardly be received in opposition to the more sure results of Mr. Hopkins.

4. With regard to Mr. Haughton's investigations, it appears to me that, owing to a fallacy in the reasoning, his conclusions do not prove anything whatever regarding the proportion of the solid to the fluid parts. He obtains the following equation of the surface of any stratum:

$$\frac{\epsilon}{a}\int_0^a \rho' a'^2 da' - \frac{1}{5a^3}\int_0^a \rho' \frac{d.a'^5\epsilon'}{da'}da' - \frac{a^2}{5}\int_a^h \rho' \frac{d\epsilon'}{da'}da' - \frac{ma^2}{2a^3}\int_0^h \rho' a'^2 da' = 0; \quad (1)$$

and by differentiation deduces $\left(\text{if we put } 3\int_0^a \rho' a'^2 da' = \phi(a)\right)$

$$\frac{d^2\epsilon}{da^2} + \frac{6\rho a^2}{\phi(a)}\frac{d\epsilon}{da} - \frac{6\epsilon}{a^2}\left(1 - \frac{\rho a^3}{\phi(a)}\right) = 0. \quad . \quad . \quad . \quad (2)$$

It is in the next five lines (p. 265) that the fallacy lies:—"This equation [viz. (2)] is identical with that derived from the supposition that the earth is completely fluid, and is therefore independent of the law of density and ellipticity of the solid parts of the earth: it determines the relation which necessarily exists between the law of density and ellipticity of the fluid portions of the earth." But this is not the case. If in equation (1) we assign different laws of density and ellipticity to the solid parts and to the fluid parts, the integrals will be discontinuous, the break being at the bounding surface. Thus the ρ' in the second and third integrals not being the same function, equation (2) does not follow from equation (1) by differentiation. In fact equation (2) assumes that the law of density and ellipticity is continuous throughout the whole mass, solid and fluid, the solid parts lying in strata of the form and density they would have if they were wholly fluid. The question is, in fact, treated by Mr. Haughton purely in a mathematical, and not in a physical manner. There is no condition or datum introduced to show what is fluid and what solid. The reasoning therefore which follows falls to the ground. The application to the case of a homogeneous body (in p. 267) proves nothing regarding solidity or fluidity. It is merely an algebraical, not a physical, problem of *densities*, and is this:—"If the earth consist of two homogeneous portions (solid or fluid, in part or in whole) bounded by a surface of equilibrium; and the outer portion have the density of superficial rock, and the nucleus such a density as to make up the whole mass of the earth; how thick is the outer portion?" The answer is 768 miles, a result which proves nothing regarding the thickness of the solid crust of the earth. The result which follows if the nucleus be not homoge-

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neous is self-evident, but, as in the former case, shows nothing regarding solidity or fluidity, but only density.

5. Mr. Hopkins's conclusion stands therefore unaffected, and, as it appears to me, is the only physical investigation of the subject upon which any reliance can be placed. The only assumption is that the earth was once fluid, or sufficiently so to have the arrangement of its parts regulated according to fluid laws. The remarkable correspondence between the ellipticity of the earth deduced upon this hypothesis and with the law of density $\frac{\sin qa}{a}$ (in itself *à priori* a very probable law), and the mean ellipticity deduced from the geodetic measurement of the surface, is an immoveable argument in favour of the hypothesis. And the near coincidence between the amount of precession observed, and that calculated from the above law of density, is an equally satisfactory argument that this law is the true one.

6. That the crust cannot be thin, as some have conceived, appears also from the following considerations. The Himalaya Mountains and the neighbouring mountain-ground may be roughly regarded as a band of superabundant matter lying on the surface of the sea-level, about 200 miles wide, 1000 miles long, and 2 miles high. The vast ocean stretching south of India may be regarded as another vast hollow of still larger dimensions, and of a general depth equal to half the mean depth of the ocean (as water is about half the density of rock), which in some parts we may take to be four miles. Now the deficiency of pressure arising from this hollow, and the excess of pressure of the mountain-region, must combine, through the intervention of the fluid or lava beneath, and produce a strain upon the crust which the crust might perhaps sustain if the strain acted by thrust and not by tension; but (as Mr. Airy has shown in the Philosophical Transactions, 1855, p. 102) no rock is sufficiently coherent or sufficiently free from cracks to withstand the *tension* which the weight of the superincumbent matter would produce in some parts of the crust, even if so thick as 100 miles. If, then, we add to this the effect of the deficiency also arising from the ocean (of which Mr. Airy takes no account), a crust as thick as 200 miles would not resist the tendency to crack and open in some parts; in which case the mountain-region would subside and the ocean-bed rise up, and the present aspect of the surface could not continue. It has been suggested that the crust may project downwards into the lava so as to be supported by buoyancy. But this will not produce the desired effect; for the crust being formed from the fluid, will have pretty nearly the same density as the parts of the lava from which it was formed;

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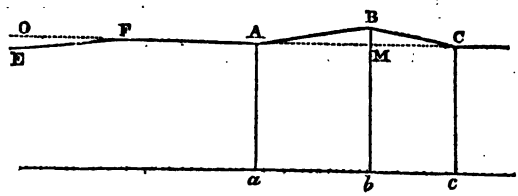
if anything, it will be somewhat more dense. Hence the superincumbent mass will have its whole effect, and the least thickness must be determined from the condition that the superincumbent weight is not sufficient to break it through.



Let E F A B C be a meridian section of the continent of India and of the ocean; A the foot of the mountains; F, Cape Comorin supposed on the same level, that is, the sea-level O F A M C. Let $AM=w$; $Aa=t$, $Bb=t'$, the thicknesses of the crust at A and B; $BM=h$. Let C be the cohesive force of the rock, or the length of rock of a unit section the weight of which equals the cohesive force on a unit of surface of the joints Aa, Bb, Cc. Now if the mass Ac sink, it will do so by opening at A, C, b; and it will be prevented sinking thus by the cohesive force of the parts of the crust at the joints. Let the unit of mass be that whose transverse section is a unit of surface. The moment of the weight of A B M about the fulcrum $a=w\times\frac{1}{2}h\times\frac{2}{3}w=\frac{1}{3}w^2h$. And the moments of the cohesive force at the joints Aa, Bb, to turn Ab in the opposite direction about $a=C\times\frac{1}{2}t^2$ and $C\times\frac{1}{2}t'^2$,

$$\therefore\ \tfrac{1}{3}w^2.h=\tfrac{1}{2}C(t^2+t'^2),\ \text{or}\ C=\frac{2}{3}\frac{w^2.h}{t^2+t'^2}=\left(\frac{w}{t}\right)^2\frac{h}{3}\ \text{nearly.}$$

Now $h=4$ miles for the Himalayas, and $w=100$ miles. Hence

$$t=\frac{200}{\sqrt{3C}}\ \text{miles.}$$

Mr. Airy states that the crust could not exert such a cohesive force as to make C so much even as one-fifth of a mile. In this case even the thickness $t=200\sqrt{\frac{5}{3}}=260$ miles.

If the same formula be applied to the ocean south of Cape Comorin, $w=1500$ miles suppose, $h=2$ miles; and therefore if $C=\frac{1}{5}$,

$$t=1\quad 5\sqrt{\frac{10}{3}}=2700\ \text{miles;}$$

or it would require this thickness to produce sufficient cohesive force to resist the tendency of the lava to break up the ocean-bed, in consequence of the deficiency of weight. If the thick-

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ness were less than this, the resultant of the cohesive force would be less, and the crust would crack at some intermediate point.

Every physical consideration seems to indicate that the crust must be very thick; and the only real calculation of its limiting thickness on physical principles, viz. that of Mr. Hopkins, should be received.

The form the surface at present exhibits may be supposed to have arisen from the contraction and expansion of the parts of this thick crust since it first began to form, producing hollows in which seas and oceans have gathered together their waters, and elevations in continents, table-lands, and mountains; and therefore the variations of the surface, under its present aspect, are not at all regulated or produced by hydrostatical principles.

J. H. Pratt.

Calcutta, Feb. 22, 1859.

LII. *A brief Account of an Induction Coil of great power in proportion to its length.* *By the* Rev. N. J. Callan, *D.D., Professor of Natural Philosophy in Maynooth College**.

In the construction of induction coils, the principal object of some seems to have been to make the coil in such a way, that with a given length of secondary wire the longest sparks may be obtained. It appears to me that it would be better to make induction coils so that, with a given battery, sparks of the greatest length may be produced. The longer the coil is, the greater will be the resistance of the primary wire to the current of the battery, and the greater the number of cells which will be required to overcome that resistance and saturate the core with magnetism. Hence it is a matter of great importance to make coils in such a way that, whilst they are short, they may produce very long sparks. I have endeavoured to do this; and though the primary and secondary coils of the induction coil I have made are very imperfect, I have succeeded tolerably well.

The primary coil is made of thick copper wire about 140 feet long: it is 10 inches in length. The conducting power of the copper wire was injured by being frequently coiled on electromagnets, or on cores of induction coils.

The secondary coil consists of three small coils: two of them are $1\frac{3}{4}$ inch long, the third is only $1\frac{1}{2}$ inch. Hence the entire length of the secondary coil is 5 inches. It is only half the length of the primary coil, and is therefore not finished. The secondary coil is made of iron wire, No. 34 gauge; the thickness of the wire is about the $\frac{1}{105}$th of an inch. The wire is covered only partially with cotton thread. Between each two ad-

* Communicated by the Author.

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joining spirals of the thread wound on the wire, there is sufficient room for another spiral of thread, and on a great part of the wire there is space enough to admit three or four threads. I adopted this mode of covering the wire in order to save time. With the same view, I arranged our machine for winding thread on wire so that by one and the same operation I covered the wire with thread and wound the wire on the coil. I fear that in many parts of the coil the bare or uncovered part of one wire is in contact with some parts of the adjoining wires. Each layer of spirals is brushed over with a hot solution of gutta percha dissolved in rosin oil. The solution is so thick, that when cool it takes the form of a paste. Each layer of spirals is insulated from those of the layer above and below it by paper saturated with the solution of gutta percha, in the manner described in the paper which I read at the Dublin Meeting of the British Association in 1857, and which was published in the Philosophical Magazine for the following November. I have in one instance seen sparks passing through the three thicknesses of paper, by which the spirals in one layer are insulated from those of the one above it. Hence the insulation of each layer of spirals from those above and below it is defective.

In trying the two parts of the coil which were first made, I observed, as often as sparks passed between the terminals of the coil, a great number of very minute sparks on the outside of one of the two parts. This made me suspect that these sparks were passing from some spirals to the adjoining ones. When I had finished the third part of the coil, I abstained from brushing over the outside spirals with the solution of gutta percha, in order to see whether sparks would pass from one spiral to another. As soon as the battery was connected with the primary coil by means of our mercurial contact-breaker, sparks passed from the bare parts of several wires to the contiguous ones. When any part was brushed over with the gutta-percha solution, the sparks ceased there, but became more numerous in some other part.

This coil, though only 5 inches long, has, notwithstanding all its defects, given sparks $4\frac{1}{8}$ inches in length with three cells of the Maynooth battery, each 4 inches square. I have not seen an account of any coil which with so small a battery has given sparks so long in proportion to the length of the coil. On account of the imperfect insulation of the secondary coil, I am afraid to use a more powerful battery.

I intend to make a new primary coil about 36 inches long, and twelve small secondary coils, each about 2 inches in length. From this coil I expect to get, with a small battery, sparks 20 or 24 inches long.

I have made several interesting experiments on the various

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parts of the coil, which I have not time at present to describe. Many more remain yet to be made. When they are finished, I shall prepare an account of them for publication in the Philosophical Magazine.

My object at present is, first, to show that iron wire, though far inferior to copper in conducting power, is not unfit for secondary coils; secondly, to direct attention to the importance of making induction coils so that with a given length, not of the secondary wire, but of the coil, the longest sparks may be produced; and thirdly, to show that a mere covering of the secondary wire with thread of any kind is not sufficient to insulate the spirals of any layer from the adjoining ones of the same layer.

Maynooth College, April 4, 1859.

LIII. *On a New Form of Telegraph Cable intended to reduce the effects of Inductive Action.* By J. N. Hearder, *Electrician, Plymouth**.

IN my last paper I described the nature of the inductive action which takes place during the transmission of electrical currents through insulated submarine conductors, and pointed out the various disturbing influences which it occasions. It is now pretty well admitted by telegraph engineers, that, unless these impediments to the free and rapid transmission of signals can be either entirely removed or considerably lessened, the commercial value of very long lines will be somewhat in the inverse ratio of their lengths. As, however, the mechanical engineer has overcome the difficulty of laying telegraph cables, it now remains for the electrician to overcome the scientific difficulties which beset his path, and to render his line of communication thoroughly efficient after it is laid.

Of late much attention has been directed to the subject, and some very able communications from practical electricians have contributed to throw much light upon it. From the very first moment when the static charge of the gutta-percha coating was brought under my notice, I felt that it would one day act as a formidable barrier to the extension of submarine lines; and I saw at that time no chance of remedy, except in the employment of larger conductors and thicker insulating coatings. Within the last year or two, plans have been proposed to reduce the amount of induction, some of which appear to be founded upon an incorrect apprehension of the electrical phænomena to which such arrangements would give rise. For instance, it has been

* Read at the Plymouth Institution, March 3, 1859. Communicated by the Author.

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proposed to include two wires in the same gutta-percha sheath, of course separated from each other by an intervening stratum of the same material, and to use one as a return wire. As this proposition has been the subject of some discussion amongst telegraph engineers, and as there are certain conditions set up by the arrangement which do not appear to me to have received sufficient attention, I have thought it necessary to offer a few remarks upon it.

The circumstances attending the action of two separate submarine cables and two insulated wires enclosed in a single sheath are altogether different. In a single cable, the whole of the internal surface of the dielectric induces upon the whole of the external surface, each being as an entirety in an opposite electrical state, thus correctly representing a Leyden jar.

In a cable, however, containing two conductors carrying reverse currents, the conditions are different, and will be represented by a Leyden jar having a glass diaphragm across its centre, dividing it, in fact, into two semi-cylindrical jars oppositely charged, in which the respective coatings of each will be partly inducing upon the respective opposed halves of the external coating, and partly upon each other through the sectional diaphragm; thus dividing the system longitudinally through the centre into two arrangements analogous in action, but opposite in character, and exerting a mutual influence on each other.

Let us now consider attentively the phænomena here involved. First, then, with regard to the portions of the two wires which include the dielectric between them. This resolves itself into a Leyden arrangement, the surfaces of whose conductors will be represented by the longitudinal sectional area of the respective wires, and whose dielectric will approximate in thickness to the mean distance between the two opposing halves of the respective wires. This portion of the dielectric, involving the action of one-half of each wire, will have a greater tendency to take up charge than the portions of the dielectric surrounding the outer halves of each wire. Now, although the wire itself may exhibit and indicate the tension of the surface with which it is in contact, yet this tension is no evidence whatever of the quantity absolutely existing as charge upon that surface, since very different quantities of electricity, exhibiting the same amount of tension, will be taken up by similar surfaces of a dielectric whose thickness varies in different parts. I will illustrate this by an example. Take a thick Leyden jar or plate, and charge it until the spark is capable of overleaping a given interval in discharge. I call the length of this discharge the degree of tension. Let the quantity of electricity necessary to produce this effect, wether measured by turns of the machine, by sparks of a given

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length from a conductor, or by discharges from a unit-jar, be assumed to be ten measures. Take now a jar or plate exposing precisely the same surface but whose glass is only half as thick, and it will be found that twenty measures will now be required to produce a discharge of the same length, or in other words, to produce the same amount of tension. Now the tension is the same in both cases, but the quantities are as 2 to 1.

Again, unite these two plates or jars so as to form a battery of double surface. Thirty measures will now be required to produce the same tension or discharge, but the relative quantities on the two plates respectively will still be as 2 to 1. We thus have an analogue of one-half of the cable; the thick glass representing the outer half, and the thin glass corresponding to the portion of dielectric between the two conductors.

It will thus be seen that one common conductor in connexion with two surfaces of dielectrics of different charging capacities will exhibit one uniform degree of tension, whilst the surfaces with which it is in contact may be taking up widely different quantities of electricity. In a double-wire cable, then, the necessary diminution of thickness between the two conductors will give rise to a correspondingly increased charging capacity of one-half of the entire charging surface.

But the mischief does not end here. It is a well-established law in electricity, and one which I have had abundant opportunities of verifying, that the effects of Leyden discharges are as the squares of the accumulated quantities irrespective of tensions; therefore whatever tends to increase the charging capacity of the surface of the dielectric, augments the mischievous effects in the ratio of the square of that accumulation: hence I cannot imagine that placing two wires in the same sheath can be anything but injurious, since one-half at least of the internal charging surface of the dielectric has its capacity to take up charge very considerably increased, by the greater facility afforded for inductive action by the diminished thickness of the intervening dielectric.

In my last communication I alluded to the advantage which might be derived from the employment of a return wire, especially of large dimensions, for the current instead of working to earth; but as I fear my remarks may have been misunderstood, I shall here enter more into detail upon that subject.

In relation to the existing Atlantic Cable, the only assistance which it could possibly afford, would be in very slightly improving the character of the earth discharge. I here avoid the term earth-circuit, because I do not believe that, except under very peculiar circumstances and within very limited bounds, any appreciable return current takes place through the

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earth. We must indeed look upon the earth as a great reservoir, capable, with proper arrangements, of affording any supply, or of disposing of any discharge. If the Atlantic Cable were a tube carrying water instead of electricity, receiving it at one end and discharging it at the other, it would never be assumed that the portions discharged in America would flow by a return current back through the ocean to supply the other end; and the cases are precisely similar: the earth acts the part of a reservoir with regard to electricity as much as the ocean does with regard to water; and the circumstances under which return-currents either of water or electricity could be produced, would bear the same analogy to each other. The only benefit, then, which the present wire could confer, would be by improving to a certain extent the conducting character of the earth, or in other words, by increasing its capacity to receive the discharge, or afford the necessary supply. I am free to admit, however, that with the present wire the amount of assistance that would be afforded might be limited from its very inadequate conducting capacity. With a capacious insulated return wire, however, the case would be different. It would, as I stated in my last paper, lessen the amount of induction by decreasing resistance, and consequently obviating the necessity of employing currents of high tension; but it must not be inferred from this that inductive action would not take place at all. The two cables would, in fact, when carrying reverse currents, represent two Leyden jars oppositely charged, and with their outer coatings in conducting communication with each other, or as is actually the case, both uninsulated. Let us now examine the various conditions of such an arrangement.

1st. Take a single cable working to earth at the remote end; the effect, however instantaneous, must originate at the battery end. If a positive current be transmitted, the wave proceeds by propulsion from the battery to the remote end, which remains normal until the excess of current over the resistance of the wire and the inductive action through the dielectric, makes its appearance there, and the effect of the discharge at that end will depend upon the relation of these counteracting elements to each other. If a negative or reverse current be transmitted, still the effect commences at the battery end by exhaustion (for want of a better term), and the remote end still remains normal until the resistance offered by the wire to the passage of electricity in that direction, and the inductive action of the dielectric, shall have permitted the effect to be manifested at the other end. In either case, the amount of the effect at the remote ends will result from a single force operating in one direction only.

2ndly. If whilst a current, say for example a positive one,

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was being transmitted from one end, the other end were at the same time brought into an oppositely electrical state by applying the negative, or, to keep up my simile, the exhausting end of a battery to it, it is quite clear that the current would pass more readily, being now under the double influence of propulsion from behind and exhaustion in front.

3rdly. Take now the case of two wires carrying reverse currents, actuated by batteries at the home end, or, what would amount to the same thing, having their home ends connected with the opposite poles of a single battery, but with their remote ends working to earth. Suppose, further, the effects to have reached the remote ends. These two ends will exhibit the characteristic conditions of the ends of the battery with which they are respectively connected, and will appear oppositely electrified, producing the ordinary galvanometric indications.

4thly. Let these remote ends be disconnected from earth, and connected with each other. We shall now have fresh conditions set up, for a positively charged insulated wire will be brought into connexion with a negatively charged one also insulated, and, according to known electrical laws, the opposite electrical states of these connected ends should, by their mutual attraction and inductive influence upon each other, still further overcome the resistance in that portion of the conductor,—the exhausting influence of the negative end accelerating the flow from the positive one, and thereby at the same time diminishing the intensity of the current, and its tendency to charge the dielectric. The two cables would now, indeed, form one continuous conductor, including, if necessary, a telegraphic instrument between them.

Now if during the passage of a current through a very extensive closed circuit of a resisting character, it shall be at all admitted that the ends nearest the battery will at any time exhibit opposite electrical conditions the relative intensities of which shall diminish towards the centre of the circuit, it follows that the telegraphic instrument between the remote ends of the two cables will be situated in the part least likely to be disturbed by inductive action. Not only would this be the case, but it would be less liable to disturbance from the residual charge of the dielectric; for if, after the transmission of a signal, the two home ends be disconnected simultaneously from the battery, and either brought to earth or closed into circuit, the whole of the actions set up to regain the normal condition will be in the direction of the two home ends, leaving the centre, viz. the point of junction of the two remote ends, comparatively free from these disturbing influences. A galvanometer at this point ought therefore to be simply influenced by the actual direction of the current in the wire, and not by the residual

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discharges from the dielectric, since any flow occasioned by them which could possibly pass through that point would necessarily be from positive to negative, or in the direction of the first current, slightly prolonging its action. The most important question, however, connected with this arrangement, and which can only be decided by experiment, is, whether the connexion of the two oppositely electrified insulated remote ends with an insulated galvanometer would not produce an equal effect with a smaller amount of current, since its velocity would be doubled. Let us consider an analogous example.

Take two Leyden jars precisely equal in capacity, that is to say, which will, with a given number of turns of the electrical machine, discharge through precisely the same interval and produce the same heating effect in the thermo-electrometer. Connect these jars respectively with the positive and negative conductors of the machine, so as to charge one positively and the other negatively. Connect with each jar a discharging electrometer, arranged so as to discharge each to its own coating through a given interval, and count the number of turns required for the effect. The two jars will discharge simultaneously, or very nearly so.

Now remove the discharging electrometers, connect the outer coatings of the jars with each other by a conductor, and connect a discharging electrometer with the two inner coatings, so that they may discharge through it to each other, the interval being made the same as before. The discharge will now pass with half the number of turns, the respective tensions of each jar mutually operating to produce the effect.

Again, make the discharging interval between the jars double; and the discharge will take place with the same number of turns as in the first case, but the spark will be twice as long. We here observe, then, that the effect of the mutual attraction of the positively and negatively electrified conductors is to facilitate the flow of electricity between them; and it is just upon this principle that I have been led to the conclusion that an insulated return wire would be preferable to an earth-circuit. Induction between the two cables, it is true, cannot be prevented; but it will be much less, as I have already shown, with two separate cables than with two wires included in the same sheath. I have been thus explicit, because the tendency of some scientific discussions which have lately taken place has been rather unfavourable to the recognition of this principle; and whilst I would not dogmatically enforce my views, yet I think they would bear the test of trial.

The success of the foregoing plan, however, would be, after all, only palliating the evil without removing or mitigating the

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cause; and I would now therefore direct attention to a new form of cable, intended to reduce not only the amount of static inductive action on the dielectric, but the degree of disturbance produced by the residual static discharge. In order fully to appreciate the value of this arrangement, it is necessary first to consider certain electrical phænomena resulting from peculiar modifications in the arrangement of conductors and dielectrics, which influence very materially the effects of inductive action; I mean statical inductive action, commonly known as the charge of the coating, in contradistinction to the dynamic inductive action resulting from the influence of a current upon an adjacent wire.

In all cases of the coated dielectrics, whether in the form of Leyden jars, coated glass or other plates, or coated wires, there are certain conditions which determine not only the amount of charge which can be taken up by a given coated surface, but the effect which the discharge of that quantity can produce. A few well-determined axioms, verified nearly thirty years since, will serve as a guide in these considerations.

1st. With any insulated charged conductor the statical intensity or power of attraction exhibited by it will be as the square of the quantity constituting the charge, and this holds good whether the conductor be simply insulated in the atmosphere, or form one of the coatings of a Leyden jar.

2nd. The thermometric effect of an electric charge, as ascertained by a wire passing through the bulb of the thermo-electrometer, either as originally invented by Sir W. S. Harris, or as subsequently modified by myself (see Phil. Mag. Nov. 1856), is as the square of the quantity transmitted, other things being the same.

3rd. With a conducting or coated surface of a given area, the length of spark or tension will be in the direct ratio of the charge. Thus, quantities of charge bearing the relations to each other of 1, 2, 3, 4, will give tensions or length of spark, as 1, 2, 3, 4; but the statical intensities as evidenced by attraction or inductive action, will be respectively in the proportions of 1, 4, 9, 16, &c., corresponding precisely to the thermometric effect.

4th. The relation of the quantity to the intensity will depend upon the surface upon which the electricity is distributed: for instance, a double surface charged to the same degree of tension, that is to say, to give the same length of spark, will contain a double quantity; but the statical intensity and inductive or attractive action will remain the same, whilst the thermometric effect will be as the square of the surface or quantity, the thickness of glass being equal.

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5th. If a given charge be distributed over conducting surfaces bearing the several relations of 1, 2, 3, 4, the thermometric effect will be unaltered, but the respective tensions or length of spark will be in the simple inverse ratio of the surfaces, viz. 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, whilst the intensities or attractive power will be inversely as the squares of the surfaces, viz. 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$.

6th. The attractive or inductive influence acting between any two electrified surfaces, is, with the same charge, inversely as the squares of the distances between which it operates. An insulated scale-pan placed over a charged conductor at the several relative distances 1, 2, 3, 4, will be balanced by different weights, these being in the inverse ratio of the squares of the distances.

7th. The capacity of a conductor to receive electric charge will depend upon the amount of induction which can take place between it and a vicinal conductor; and the nearer these can be brought together, one being uninsulated and the other insulated, the greater will be the amount of charge.

Now, as there can be no doubt that it is the dielectric in contact with the conductors which receives the charge, the conductors serving only to distribute or collect it, we may in the preceding law lose sight of the conductors beyond this function, and merely consider that the dielectric has its capacity for receiving charge increased by diminishing its thickness. The following then appear to be some inferences to be deduced from the action of these laws.

1st. Since resistance in wires requires the employment of intensity in currents to overcome it, it follows that the greater the intensity of the current, that is to say, the greater the amount of free statical electricity accompanying the dynamic current, the greater will be the tendency to charge the dielectric. Now any increase of charge on the dielectric will produce results in discharge as the squares of the quantities thus taken up. Upon this principle I propose to use a capacious conductor, which shall diminish resistance as much as possible, and thereby reduce correspondingly the intensity of the current requisite to work through it, and its tendency to charge the dielectric.

2nd. Since the quantity of charge which can be taken up by any dielectric will depend upon its thinness, or the contiguity of the conducting surfaces between which it is included, I propose to increase the thickness of the dielectric or insulating coating, and thereby not only still further reduce its tendency to charge, but increase its insulating faculty. There are, however, other conditions which interfere still more with the results of discharges from coated surfaces which have been hitherto overlooked, but which may nevertheless render essential service in their judicious application to the insulation of a submarine conductor.

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If a Leyden jar or a coated glass plate have layers of paper or any other fibrous material inserted between either or both of its coatings and the glass, although the glass may, as far as the phænomena of tension or attraction are concerned, be charged to the same intensity, yet the effects of the discharge suffer an extraordinary diminution, as will appear by the following experiments. (Coated glass plates afforded the most ready means of carrying out this inquiry, and were consequently employed for the purpose. The effects of the discharge were examined by means of the thermo-electrometer before described.)

1st. To ascertain the effect of interposing porous media between the glass and its coatings, various substances were employed, such as silk, flannel, calico, paper, &c. The effect of the discharge from the glass plate, with its two ordinary tinfoil coatings, was first ascertained by the thermo-electrometer; the tinfoil coatings were then removed, and the several porous substances were in turn placed upon the glass, varying from 1 to 3 layers in thickness, either on one or both sides of the glass; and, the coatings being carefully replaced upon them, the glass was again charged, and the effects upon the electrometer noticed. Without going into detail upon the results of each modification of the arrangement, it may be sufficient to state that, whilst with the ordinary tinfoil coatings a given effect was produced, only $\frac{1}{10}$th to $\frac{1}{100}$th of the effect was obtained when the porous layers were interposed; and this diminution of effect bore no apparent relation to the conducting or non-conducting character of these media.

In order to ascertain if these losses were occasioned by the greater distance to which the tinfoil coatings were removed, glass plates of a thickness corresponding to the united thickness of the glass plates and fibrous coatings before mentioned were employed, and coated with tinfoil in contact with the glass; but these gave results bearing their correct relation to the difference in the thickness of glass when coated with tinfoil alone, the results being very much higher than those obtained by the intervention of the porous media. The quantity of electricity thrown on as charge was accurately measured in each experiment, and the discharging interval was retained the same.

Thus the tension underwent no change; the intensity, as evidenced by attraction, was more rather than less; and yet the thermometric corresponding to the galvanometric effects in wires suffered this extraordinary diminution.

Again, the arrangement was modified by using two plates of glass placed upon each other as a double dielectric; tinfoil coatings were placed upon the two outer surfaces, and porous layers inserted between the plates. The reduction of effect was here

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still more marked; for, although considerable thermometric effects were produced by the two plates of glass enclosed between the tinfoil coatings, yet no appreciable effect could be obtained with the same charge when porous layers were inserted between them.

Reasoning upon these results, it occurred to me that, in addition to the advantages before alluded to, to be derived from increasing the thickness of the dielectric and the capacity of the conductor, still further benefit would result by adopting some modification of the foregoing arrangements in relation to the cable. The interposition of porous or fibrous media between the coatings and the jar or glass plate, reduces the effects of the discharge; and since it is the discharge, more than the charge, which produces the embarrassment of signals, it appears a fair deduction that any arrangement which will reduce the effect of this discharge, will to the same extent diminish the amount of mischievous influence.

Four modes of carrying out the conditions of the foregoing experiments with regard to the cable, presented themselves.

1st. By inserting a porous layer between the dielectric and its inner coating (the wire), which could be done by covering the wire with cotton, silk, or worsted, or any fibrous substance, previously to coating with gutta percha.

2nd. By putting a fibrous coating of this kind over the gutta-percha covering of a wire already insulated in the ordinary way, taking care, of course, to make this coating impervious to moisture.

3rd. By inserting the porous or fibrous media between the layers of gutta percha; or

4th. By adopting two or more of these processes in conjunction with each other.

With regard to the first process, I found from actual experiment that when the conducting wire was very small, as, for instance, in the Atlantic Cable, the application of a porous covering to it previously to the gutta-percha coating unduly increased its bulk in proportion to its original size, and thereby proportionally increased the inner charging surface of the gutta percha. But even under the most unfavourable circumstances, when for the sake of experiment a double coating of fibrous material had been put on, and the inner surface of the gutta-percha increased to nearly three times its original dimensions, whilst the thickness of the gutta-percha coating itself was less, thereby enabling it to take up more than double the amount of static charge, yet the effects of the discharge were no more than from the original surface in contact with the wire; so that if only the same amount of charge had been thrown in, less than half the original effect might have been expected.

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Since, however, according to the experiments with the glass plates, the porous material was equally effective on the outside, or between two plates, it would appear that the most eligible situation for the fibrous layer in the cable, would be either between the coatings of gutta percha or externally to them; and I doubt not that in either of these situations it would act with the cable, as with the glass plates, in materially reducing the effects of the discharge.

There are also other considerations of a mechanical character which render these modifications of the arrangement preferable. I venture then to recommend, with considerable confidence, the adoption of this plan as a simple and efficient mode of greatly reducing—I will not go so far as to say entirely obviating—the embarrassing influences of the residual discharge.

In a word, a cable constructed upon the principles here laid down will possess the following electrical qualifications, viz.—

Its conductor will be of ample dimensions, thereby reducing the tendency to produce static induction.

The dielectric or insulating coating will also, by its increased thickness, not only improve the insulation, but have a diminished capacity for taking up inductive charge; and, lastly, the insertion of the fibrous substance will go very far to obviate the mischievous effect of any amount of charge that may be taken up, since by analogous arrangements we find that from 80 to 90 per cent. of this influence may be intercepted.

I cannot close this paper without alluding to the mechanical advantages also possessed by this form of cable. It is well known that when the gutta-percha coating of a cable is subjected to great strain, small fissures, before imperceptible, gradually become apparent from the separation of surfaces between which there was previously a want of perfect cohesion, although without this strain the cohesion might have been sufficient for electrical insulation, and the fissures might not have been formed. I have no doubt that the Atlantic cable has suffered in this manner, since it had no provision for preventing the gutta percha from stretching.

Propositions have lately been made to form the core in such a manner as to take all the strain; but independently of the most serious electrical objections to a conductor compounded of iron and copper, I venture to predict that whenever such a cable shall be subjected to the strain of passing over pulleys, the gutta-percha coating will separate into pieces and slide over its included conductor. The interposition, however, of flax or hemp strand, which is a very eligible material, and which may be either braided on or laid on in a very long twist between the layers of gutta percha and incorporated with them, will communicate such

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an amount of mechanical strength to the insulating coating, that, in addition to its improved electrical character, it will be free from the disadvantages before alluded to, when not thus protected. The specific gravity also of such a cable may be made, if necessary, extremely low.

When made of the requisite dimensions, it need not weigh more than 6 cwt. per mile in air, or 1 cwt. in water; and, from experiments made with a sample of an analogous construction, the breaking strain would be upwards of 17 cwt., or equivalent to between seventeen and twenty miles of its own length in water. It will be remembered that the Atlantic Cable would not support three miles of its own length in water; and I question much if the integrity of its gutta-percha coating could be depended upon under the strain of a single mile.

These are considerations which, I would venture to submit, are of no mean importance, even when viewed apart from any electrical qualities, but which, when taken in connexion with those already described, appear to me to give to this form of cable a decided superiority over any form yet devised. I say nothing of any subsequent coating of wire, &c. for additional protection in exposed situations, when found to be necessary, as such an arrangement is as applicable to this form of cable as to any other.

LIV. *Note as to the relation of Common and Voltaic Electricity.* By J. J. Waterston, *Esq.**

IN the seventh series of his 'Experimental Researches,' Faraday treats of the absolute quantity of electricity associated with the atoms of matter, and sums up with a statement as to the quantity of electricity associated with the chemical elements of a grain of water, which has often been quoted since in a way that tends to mislead as to the potential magnitude of the forces involved. There is an example of this in the last (January) Number of the Edinburgh Review, p. 235, where the following passage occurs:—"Yet they find authority in the marvellous fact, well authenticated by Faraday, that one drop of water contains, and may be made to evolve, as much electricity as under other manner of evolution *would suffice to produce a thunderstorm.*" The mechanical value of the chemical force that unites the oxygen to the hydrogen of a grain of water is well known to be about equal to the weight of 7 cwt. exerted through one foot. That such amount of force would *suffice* to produce a thunderstorm is plainly an idea that cannot be entertained; nor is it strictly implied by the words of Faraday, which are—"The che-

* Communicated by the Author.

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mical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm." This was written in 1833, when the application of a mechanical standard or work equivalent to molecular forces was but little thought of. To avoid conveying an incorrect impression now, it would be necessary to underline the word "quantity," and add, "but of *incomparably less intensity* than that of a powerful thunder-storm." The idea of the mechanical equivalent of a grain of water being equal to the mechanical equivalent of a thunder-storm would thus be excluded.

The progress of science, and the labours of Harris especially, has since enabled us to obtain some clearer ideas of quantity and intensity as applied to electricity. When the mechanical value of a constant quantity under different degrees of intensity has been ascertained—and this seems practicable with Harris's apparatus—we shall be in a position to estimate exactly the potential relation between voltaic and common electricity. In the mean time it may be useful to direct attention to certain data which already exist, by means of which we may roughly calculate an approximate result.

The great electric battery of the celebrated Dutch electrician, Van Merum, consisted of 100 jars, each exposing $5\frac{1}{2}$ square feet of coated glass, making altogether 550 square feet. It is stated that this battery, discharged through a length of 25 feet of iron wire $\frac{1}{140}$th of an inch in diameter, fused it so that it was converted into red-hot balls thrown in all directions. Assuming that the heat evolved was sufficient to raise the temperature of the wire 3000 degrees, we have $\frac{1}{52}$ of a cubic inch of iron thus heated; this is equivalent to about $\frac{1}{58}$ cubic inch of water raised 3000 degrees, or 15,000 grains raised 1 degree.

To compare this battery strictly with that employed by Faraday, we should require to know the electric tension of each when charged, as indicated by the same electrometer; also the thickness of the glass in each. Such data are wanting; but, for a rough estimate, we may perhaps assume that they did not differ materially in these particulars.

Faraday states that the quantity of electricity required to decompose a single grain of water is equal to 800,000 charges of a 25 square feet battery, each charge made by thirty turns of a plate-glass machine, 50 inches diameter, in full action. The product of 25 by 800,000 is 20,000,000 square feet of coated glass. This, compared with 550, shows that the quantity of electricity associated with 1 grain of water is upwards of 36,000 times the amount in Van Merum's battery, and consequently the heating power must be equivalent to 15,000 grains of water raised 36,000 degrees.

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According to the experiments of MM. Dulong and Hess, 1 litre, or 61 cubic inches of hydrogen, burned in half a litre of oxygen, gives out heat sufficient to raise 3102 grms. of water 1° C., or 12·3 lbs. 1° F. The water formed by the combustion is 0·00168 lbs. The ratio this bears to 12·3 lbs. is 1 to 7345; so that the heat given out by the combustion of hydrogen sufficient to form 1 grain of water, would raise 7345 grains of water 1° F. in temperature. Comparing this with 15,000 grains raised 36,000 degrees, we arrive at the conclusion that the mechanical value of the electricity required to decompose 1 grain of water is less than $\frac{1}{76,000}$th of the mechanical value of the electricity in the 800,000 charges of the 25-feet battery. Thus eleven charges of this battery represent the integral electric force contained in 1 grain of water.

Edinburgh, February 13, 1859.

LV. *Note to a Letter "On the Conservation of Energy."* By W. J. Macquorn Rankine, *C.E., LL.D., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG leave to add to my letter which appeared in the Philosophical Magazine for April, the following remarks relative to the terms "Force," "Power," or "Energy," when used in the sense of *an unknown secondary cause* of physical changes.

The magnitude of a cause, if measurable at all, can only be measured indirectly by means of the magnitude of its effects. The magnitude of the cause may be a simple quantity; but the effects by which it is to be measured may be of such a nature, that, to obtain from their magnitudes a proper measure of the magnitude of the cause, factors must be multiplied together, and terms added together. The tests of a proper measure are, first, its "*conservation*," that is, the invariability of its magnitude by any actions amongst the system of bodies to which it relates; and secondly, its *completeness*, or its taking into account all the known relations amongst those bodies.

To measure the magnitude of the "energy" of gravitation in a system of bodies, not only must the *mean attraction* between each pair of them be multiplied by the distance through which it is *capable* of acting (giving what I have called the "*potential energy*"), but the mass of each body must be multiplied by the half-square of its velocity relatively to the centre of gravity of the system (giving what is called "actual energy" or "*vis viva*"), and the results must be added together. The quantity thus measured is not the less a simple magnitude

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because the phænomena by means of which it has to be measured are of such a kind that their magnitudes have to be multiplied and added in order to complete the measurement required,—just as the volume of a body is a simple magnitude, although to ascertain it we have to measure the three dimensions of each of several parts into which we divide that volume, multiply them together, and add the products.

Momentum and angular momentum possess, like energy, the property of *conservation*; but they do not possess the property of *completeness*, inasmuch as they take into account actual motions only, and not tendencies to move. Attraction takes into account tendencies to move only, and not actual motions. Energy alone takes all those relations into account.

The attempt to measure the "force," "energy," or "power" of a system of gravitating bodies (by what term soever the quantity to which the law of conservation of energy applies is designated) by taking into account only the attractions, or *tendencies* to mutual approach of the bodies, would be analogous to the attempt to measure the volume of a body by means of one dimension of one of its parts.

It appears to me that the consideration of the facts I have mentioned tends to show that there is less real difference between the opinions of Dr. Faraday and those of some other writers on this subject than may at first have been supposed.

I wish it to be understood, that nothing in these remarks is to be construed into an expression of any opinion on any metaphysical question relative to the nature of physical causation.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, April 13, 1859. W. J. Macquorn Rankine.

LVI. *On the Connexion between the Structure and the Physical Properties of Wood.* *By* Prof. Knoblauch*.

THE author seeks to ascertain *whether any connexion is ascertainable between the structural relations of various kinds of wood and their observed physical properties, such as their powers of resonance and conduction of heat, &c.*, in the same way as was done for one and the same wood by Savart in respect to resonance, and more especially by Tyndall in respect to the conduction of heat.

The primary object was to trace the difference in the conduction of heat shown by different woods, according as the heat has to traverse the wood in a direction parallel with, or at right angles to, the direction of the grain. For this purpose, slabs of the woods to be examined were bored through perpendicular to

* Translated by Dr. F. Guthrie, from the *Sitzungsberichte der naturforschenden Gesellschaft*. Halle, 1858. vol. v.

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their planes, and then covered as uniformly as possible with a coating of stearine. A hot wire, exactly fitting the bore, was introduced into the latter and continually turned round during the experiment. By this means the coating of stearine around the orifice was melted; but, as we should expect, not in concentric circles, but in elliptic zones, whose major axes invariably coincided with the direction of the grain. The great difference in the behaviour of different kinds of wood (about eighty sorts were examined) under these circumstances is at once apparent. With some the ellipses are tolerably circular, by others more elongated, while by others, again, the major axes are so extended as to be nearly twice the length of the minor ones. The excentricity of these ellipses, which furnished a graphical expression for the conductive power of the wood in the directions between which the structural difference was greatest, made it possible to divide the different kinds of wood into four distinct groups. In the first, the ratio of the minor to the major axis of the ellipse is on the average as 1 to 1·25. To this group, Acacia, Box, Cypress, King-wood, &c. belong. In the second, and by far the most numerous group, containing Elder, Nut, Ebony, Apple, several dye-woods, &c., the mean value of this ratio is 1 to 1·45. In the third group, to which Apricot, Siberian Acacia, Brazil wood, Yellow wood from Puerto Caballo, &c. belong, the ratio is as 1 to 1·60. In the fourth group it is as 1 to 1·80, and to this division belong Lime, Tamarind, Iron-wood, Poplar, Savanilla (yellow), &c. Hence *the conducting power of all woods in the direction of the fibre exceeds that in the perpendicular direction by no means in a constant manner, but in one which depends upon the nature of the wood.* This superiority is in the first group so small, that the warmth in the direction of the fibre traverses a path only a quarter more in length than that traversed in the same time in a perpendicular direction. In the last group, on the other hand, the length of the path in the first direction is about twice that in the perpendicular one.

In order to investigate the relations of resonance, two rods were cut from each kind of wood,—the one being taken in the direction of the grain (Langholz), the second perpendicularly across it (Hirnholz). On suspending these rods freely (their length was 470 millims., breadth 20 millims., and thickness 8 millims.) and striking them with a stick, the piece cut with the grain always gives a more sonorous tone than the corresponding cross-grain piece. Nevertheless the difference of resonance in the tones of the with- and cross-grain pieces of one and the same wood, of the first of the groups described (say beech), is unmistakeably less than the difference between the tones of the with- and cross-grain pieces of any member of the second

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group. In the second group this difference is less than in the third; and in the third, again, less than in the fourth (as with with- and cross-grain pieces of poplar). *When, therefore, the fibres of all kinds of wood are set in vibration, the purity of resonance is greater when such vibrations are transverse than when they occur in other directions* (*as when the rods are cut across the grain*). *But this superiority of resonance is not constant; it depends upon the nature of the wood.* The difference in this respect in the first group of woods is so small, that the resonance of two with- and cross-grain pieces resembles that of two not very dissimilar masses of stone when struck. In the last group the difference is so great, that the tone of the with-grain piece when struck has a metallic ring, while the dull sound of the cross-grain piece reminds one of a piece of pasteboard when struck. *The division of the woods examined, derived from their thermo-conductive power, is accordingly supported by their acoustic relations.*

By supporting the two ends of the rods employed in the above experiments and loading them equally in the middle, the degrees of deflection which they undergo will give us an insight into their structural relations; for the greater their compactness, the greater the resistance they will offer to bending; and the less compact they are, the more easily will they yield. The difference in vertical height of the middle points of the bent and straight rods was taken as measure of deflection. A lever was employed to determine this measure, the end of which passed over an enlarged scale in order that the readings off might be the more exact. The unit of this measure was a matter of indifference, inasmuch as in the comparison to be instituted, relations only had to be determined. Although, as was to be expected, in all cases the with-grain piece was much less flexible than the corresponding cross-grain piece, yet an important difference was noticeable in the different groups. This is best seen by calculating the relation between the bending (measured as above described) of the with-grain and that of the cross-grain wood; that is, the same weight being applied (say 100 grs.), by dividing the number given by the lever with the cross-grain piece by that given with the with-grain piece. This relation (called "ratio of deflection" in the following Table) has, in the first group, the mean value of 1 to 5; in the second, 1 to 8; in the third, 1 to 9·5; in the fourth, 1 to 14. The division of the groups is therefore also supported from this point of view*. *The difference in the structure in the different directions is least in those woods which show the least difference with respect to direc-*

* The diversity of nature, even with one and the same kind of wood, of course did not admit of the boundaries of the groups being drawn with great exactness, or of the subdivision of the groups into secondary ones.

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tion in their thermo-conductive and resonant properties; and the difference in the former is greater or less as the two latter differences are greater or less.

Hence a definite relation may be established between the different phænomena described; and this is true to such an extent, that the knowledge of one of them, e. g. *the mechanical or state of cohesion, is sufficient to deduce the others, those of warmth or resonance.*

Thus, merely to adduce one example, especial experiments had shown that in petrified woods a difference of structure in the directions parallel with, and perpendicular to, the direction of the grain had been preserved; and, in fact, the thermal curve was an ellipse whose major axis was parallel to the fibres. As in the petrified example this difference in mechanical structure was much less than in the living wood, so also, while in the living Conifer the ratio of the axes was as 1 to 1·80, in the petrified specimen it had sunk to 1 to 1·12.

The following Table contains the names of the woods examined, arranged according to the groups mentioned:—

Group I.

Ratio of the axes of the thermal ellipse 1 to 1·25. Mean ratio of deflection 1 to 5·0.

Acacia.	King-wood.
Box.	Satin-wood.
Lignum-vitæ.	Salisburia (*Gingko*).
Cypress.	

Group II.

Ratio of axes of thermal ellipse 1 to 1·45. Mean ratio of deflection 1 to 8·0.

Elder.	Snake-wood.
Alder.	Zebra-wood.
White Thorn.	Purple-wood (*Amaranthus*).
Arbor vitæ.	Settin.
St. Lucien wood.	Coromandel wood.
Gymnocladus canadensis.	Angica-wood.
Beech (2 species, white and red).	Cocoa-wood (*Gateado*).
Plane.	Apple.
Elm.	Pear.
Oak (two species).	Cherry.
Ash.	Plum.
Maple.	Sandal (red).
American maple.	Caliatour.
Cedar of Lebanon.	Costarica (red wood).
Australian cedar.	Bimas sapan.
Mahogany.	Cuba (yellow wood).
Palisander.	Viset (yellow wood).
Ebony.	Campeachy blue wood.
Palm.	Tabasco blue wood.
Rosewood.	Domingo blue wood.

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Group III.

Ratio of axes of thermal ellipse 1 to 1·50. Mean ratio of deflection 1 to 9·5.

Apricot.	Pernambuco red wood.
Pistachio.	Japan red wood.
Siberian Acacia.	Puerto-Caballo yellow wood.

Group IV.

Relation of the axes of the thermal ellipses 1 to 1·8. Mean ratio of deflection 1 to 14·0.

Willow (two examples).	Weymouth fir.
Chestnut (three examples).	Magnolia.
Lime.	Iron wood.
Alder.	Tamarind.
Birch.	Palmassu.
Poplar (three examples).	"Kistenholz."
Aspen.	Caoba (Havannah Cedar).
Pine.	Savanilla yellow wood.
Fir.	

LVII. *On the Resistance of the Luminiferous Medium to the Motions of Planets and Comets.* *By* Professor Challis*.

KEEPING still in view the investigation of a mathematical theory of physical forces on the hypothesis of the dynamical action of a continuous elastic fluid pervading space, I propose, as a preliminary step, to give reasons for assuming the existence of such a medium, and to endeavour to obtain as distinct an idea as possible of its nature and properties. As this inquiry is closely connected with the undulatory theory of light, the principles of that theory, and the indications it gives of the existence and qualities of the medium by which light is supposed to be transmitted through space, necessarily come under consideration. But besides that the medium must be such as to account for the various phænomena of light, it must also be consistent with other classes of facts to which it can be shown to be related. For instance, since physical astronomy has proved that the planets are not perceptibly acted upon by any other force than gravitation, we must show that the luminiferous medium offers no sensible resistance to their motions. To reconcile this fact with the properties of a medium, which has been supposed to give rise to the phænomena of light by the motions of its *separate* atoms, is acknowledged to be a difficulty by the advocates of that kind of agency. In a communication to the Philo-

* Communicated by the Author.

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sophical Magazine for December 1853 (p. 404), Mr. Rankine makes the following statement:—

"It has hitherto been always assumed that the kind of motion which constitutes light is a vibration from side to side, transmitted from particle to particle of the luminiferous medium, by means of forces acting between the particles. In order to account for the transmission of such transverse vibrations, the luminiferous medium has been supposed to possess a kind of elasticity which resists distortion of its parts, like that of an elastic solid; and in order to account for the non-appearance in ordinary cases of effects which can be ascribed to longitudinal vibrations, it has been necessary to suppose further, that this medium resists compression with an elasticity immensely greater than that with which it resists distortion; the latter species of elasticity being, nevertheless, sufficiently great to transmit one of the most powerful kinds of physical energy through interstellar space with a speed in comparison with which that of the swiftest planets of our system in their orbits is appreciable, but no more.

"It seems impossible to reconcile these suppositions with the fact, that the luminiferous medium in interstellar space offers no sensible resistance to the motions of the heavenly bodies."

It seems also inconceivable that a definite and permanent arrangement of the ætherial atoms, which is an essential condition of the only *à priori* investigation of transverse vibrations which the supporters of these views have given, can consist with the free motion of the heavenly bodies through the æther. To meet these difficulties, the author of the above extract proposes an hypothesis of oscillations, which will be best stated in his own words:—"The hypothesis now to be proposed as a groundwork for the undulatory theory of light, consists mainly in conceiving that the luminiferous medium is constituted of detached atoms or nuclei, distributed throughout all space, and endowed with a peculiar species of polarity, in virtue of which three orthogonal axes in each atom tend to place themselves parallel respectively to the corresponding axes in every other atom; and that plane-polarized light consists in a small oscillatory movement of each atom round an axis transverse to the direction of propagation."—(P. 406.) "It is evident that how powerful soever the polarity may be, which is here ascribed to the atoms of the luminiferous medium, it is a kind of force which must be absolutely destitute of direct influence on resistance to change of volume or change of figure in the parts of that medium, or of any body of which that medium may form a part; and that, consequently, the difficulty which in the hypothesis of vibrations arises from the necessity of ascribing to the luminiferous medium properties like those of an elastic solid, has no existence in the

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hypothesis of oscillations now proposed."—(P. 407.) This theory is accordingly distinguished from that commonly maintained, by the substitution of oscillations of the individual atoms about axes for their vibrations in space. Here I take occasion to remark, for the sake of preventing misapprehension, that where in my former communications I have spoken of the oscillatory theory of light (which ought rather to have been called the vibratory theory), I made no reference to the particular views adduced above, intending only to signify by those terms a theory which takes account of the motions of the discrete atoms of the æther, and is in this respect opposed to the hydrodynamical theory. In referring now to this modification of the usual theory, my object is the same that I had in view when referring in my last communication to the theory of elliptically-polarized light,—namely, to exhibit the actual state of the theory of light maintained by the mathematicians of the present day, and to show by their own admissions that it requires hypothesis to be added to hypothesis to support it. I have no doubt in my own mind that it involves the initial and radical defect of treating by common differential equations, questions which demand the more comprehensive analysis of *partial* differential equations. It is this mode of treatment that necessitates the adoption of hypotheses, respecting the molecular constitution of the æther, which at best must be precarious, and which absolutely admit of no verification, if, as there is reason to conclude from certain analogies of light to sound, the phænomena of light result from the dynamical action of a *continuous* fluid. As the laws of sound can be investigated only by means of partial differential equations, it seems scarcely possible that this instrument of research can be dispensed with in investigating the laws of light.

In confirmation of the above views, I proceed now to give an explanation of the non-resistance of the æther to the motions of the heavenly bodies, by regarding it as a fluid substance the movement of which is ascertained by the integration of partial differential equations. For this purpose it is not necessary, nor indeed should I consider it allowable, to make any other hypotheses than those which I have already made, viz. that the constituent atoms of the bodies which move in the æther are hard, inert, and spherical; and that the æther itself is a continuous and highly elastic fluid, varying in pressure in proportion as it varies in density. The question consequently resolves itself into the determination of the resistance of the æther to the motion of a minute spherical body. This problem I have considered in a communication to the Philosophical Magazine for July 1832, (p. 40), and I do not yet see any reason for treating it in a different manner. A solution of the same problem is also given in the

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e Philosophical Transactions, vol. vii. part 3, in a paper Motion of a small Sphere in an Elastic Medium," the former of these solutions the conclusion arrived stated:—"When a small sphere moves in a medium with a velocity small compared to the velocity of propagation in the medium, and in any manner except in rapid vibrations, the pressure on its surface is at every point very little different from the pressure of the medium at rest." Having made these references, I think it unnecessary to adduce the analytical reasoning in detail: it may suffice for the present purpose to indicate the principles on which it proceeds, which are as follows. In an article "On the Central Motion of an Elastic Fluid," contained in the Philosophical Magazine for January 1859, I have obtained the following general values of the velocity V and condensation S at the distance r from the centre at any time t:—

$$V = \frac{1}{r}\Sigma . \left\{ m \cos \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\},$$

$$- \frac{1}{r^2}\Sigma . \left\{ \frac{m\lambda}{2\pi} \sin \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\},$$

$$\kappa a S = \frac{1}{r}\Sigma . \left\{ \pm m \cos \frac{2\pi}{\lambda} (r \mp \kappa a t + c) \right\}.$$

The ratio of the coefficients of corresponding terms in the two groups of terms that make up the value of V is $-\frac{2\pi r}{\lambda}$, which, for a given value of r, is smaller as λ is greater and the vibrations less rapid. Hence as V approaches to uniformity for a given value of r, this ratio becomes indefinitely small, and the first group of terms vanishes in comparison with the other. Hence also the value of aS is of insensible magnitude; and the effective pressure, which varies as a^2S, may be exceedingly small notwithstanding the large factor a, when, in consequence of the motion being nearly uniform, the ratio $\frac{r}{\lambda}$ must be exceedingly small for a given small value of r. If the fluid be supposed to be disturbed by the motion of a small sphere, the velocity impressed by the sphere is at each instant directed to or from its centre, because, being perfectly spherical, it is incapable of impressing motion in any other directions. Now from the principle of rectilinear axes of propagation (which results from reasoning given in a paper "On a New Fundamental Equation in Hydrodynamics," in the Cambridge Philosophical Transactions, vol. viii. part 1, art. 7, and in a communication to the Philosophical Magazine for December 1852, Prop. X.), it follows that this

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impressed motion is rectilinearly propagated from the moving centre. Hence if θ be the angle which any radius makes with the direction of the motion, the above expressions for V and S are applicable to this case if the right-hand side of the equations be multiplied by $\cos\theta$. Hence it may be inferred, as above, that in proportion as the motion of the small sphere tends to be uniform, the value of a^2S becomes insensible. If the analysis had embraced the *square* of the velocity, no resistance would have been found, because the velocities impressed by the preceding and following halves of the sphere are equal with opposite signs, and consequently the pressures on the opposite hemispheres, so far as they depend on the square of the velocity, destroy each other.

These considerations seem to justify the inference, that a planet composed of discrete atoms of the kind assumed above, might pass through the æther and put it in motion without suffering sensible retardation. Any sensible resistance of the æther would first become apparent in the case of a comet moving in an excentric orbit with a rapidly varying velocity. As the above explanation of the non-resistance of the luminiferous medium to the motions of the planets essentially depends on the supposed spherical *form* of the atoms, it is so far a confirmation of the truth of the supposition. It may also be remarked that, according to the principle of the coexistence of small vibrations, the motions impressed by the atoms disturb in no respect the transmission of light-undulations in the neighbourhood of a planet or comet.

Cambridge Observatory,
April 20, 1859.

LVIII. *Observations on the Theory of Equations of the Fifth Degree.* By James Cockle, *M.A., F.R.A.S., F.C.P.S.* &c.*

[Continued from vol. xiii. p. 364.]

34. LET us apply the foregoing discussion to the trinomial

$$x^5+A_3x^2+A_5=0,$$

by means of the auxiliary equation

$$x^3+px+p_2=y.$$

35. If we eliminate x, the quintic in y will be solvable provided the resolvent product $\theta(y)$ vanishes.

36. The development of

$$\theta(y)=\theta(px+x^3)=0$$

* Communicated by the Author.

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is the biquadratic, in p,

$$\theta(x) \cdot p^4 + 5 \cdot \tau(x^2) \cdot \chi(x) \cdot p^3 + \{\theta(x^2) - \sqrt{5} \cdot \tau(x) \cdot \chi(x^3)\} p^2 + 5 \cdot \tau(x^2) \cdot \chi(x^3) \cdot p + \theta(x^3) = 0,$$

in which τ and χ are defined by

$$\tau(u) = u_1u_2 + u_2u_3 + u_3u_4 + u_4u_5 + u_5u_1,$$

$$\chi(u) = 2\tau(u) - \Sigma u_1u_2.$$

37. But, if

$$F_1(u) = \Sigma u^2 + (i^2 + i^3)\Sigma u_1u_2 + \sqrt{5} \cdot \tau(u),$$

$$F_2(u) = \Sigma u^2 + (i + i^4)\Sigma u_1u_2 - \sqrt{5} \cdot \tau(u),$$

we are led to

$$\{F(u)\}^2 - (2\Sigma u^2 - \Sigma u_1u_2)F(u) + \theta(u) = 0.$$

38. Consequently, if θ can be determined generally, F, τ, χ, p, y, and x will be known.

39. The algebraic roots, if any exist, of the above trinomial are all included in the expression

$$x_m = \tfrac{1}{5}\left\{ i^m X + i^{2m} X_2 - \frac{\sqrt{-\theta(x)}}{i^{2m} X_2} + \frac{\sqrt{-\theta(x)}}{i^m X} \right\},$$

as those of the general quintic are in the corresponding formula

$$\tfrac{1}{5}\left\{ A_1 + i^m X + i^{2m} X_2 + \frac{F_2(x)}{i^{2m} X_2} + \frac{F_1(x)}{i^m X} \right\}.$$

40. Hence, instead of proceeding by the above transformation, we may, when $\theta(x)$ is known, seek X by means of the relations given in art. 36 of Euler's paper, to which I have already* referred.

40. In a paper which will be printed in the forthcoming (fifteenth) volume of the Manchester Memoirs, I have given the coefficients of the equation in $\theta(x)$ when the given quintic is of the above trinomial form. Mr. Harley has verified them all, and has devised a striking process of calculation, of which I have availed myself in obtaining some of the above results. For the expression $\tau(x)$ in the general case I am indebted to Mr. Harley, who brought it under my notice in a correspondence to which my paper gave rise. At first I had employed, in particular instances only, an operation equivalent to F.

4 Pump Court, Temple, E.C.,
April 26, 1859.

* Vol. xv. pp. 389–90, where the S which occurs in the symmetric product equals $P^2 + R$.

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LIX. *Notes on certain Vibrations produced by Electricity. By* J. D. Forbes, *Professor of Natural Philosophy in the University of Edinburgh*.*

In the course of last summer (1858), I became acquainted with a phænomenon described by Mr. Gore in the Philosophical Magazine for June (Supplement, p. 519), of the following nature:—A metal cylinder, supported on two metallic rods or rails, the latter being in connexion respectively with the poles of a battery, revolves in either direction, at will, under the action of an electric current copious in quantity. Also continuous rotation of a light copper ball, supported on two circular metallic rails, takes place in either direction at pleasure, depending on the first impulse. It appeared to me very probable that this interesting fact might be applied to explain what is still obscure in the experiment on heated metals, generally known as the "Trevelyan Experiment," described by Mr. Trevelyan in the Edinburgh Transactions, vol. xii., where there is also a paper by myself on the same subject. With a view to elucidate the experiment, I had Mr. Gore's circular railway and ball constructed some months since by Mr. Kemp. I had not an opportunity of seeing it tried until October 19, when I found it to answer well with four Bunsen's pairs connected for quantity. The same day, in Mr. Kemp's laboratory, I laid a brass "Trevelyan" bar or rocker on the edge of the brass plate forming the outer rail of Mr. Gore's machine, and connecting the rail with one pole of the same battery, and the bar (by means of a globule of mercury inserted in a cavity in its upper surface) with the other, energetic vibrations commenced quite resembling those occasioned by heat in the ordinary form of the experiment on a leaden support.

I have since found, among other results,—(1) That the vibration goes on in whichever direction the electric current passes. [At first I thought that there was a superior effect when the current passed from a good to an imperfect conductor; but this has not been confirmed, as far at least as I have gone.] (2) The vibrations take place both between metals of the same kind and heterogeneous metals. (3) When heat is applied to a brass bar vibrating on cold lead, and then electricity is applied as before, the effects are superadded to one another, whichever way the current passes, the vibrations becoming more energetic; and if there be a musical note, it becomes graver [owing, it is assumed, to the increased arc of vibration]. (4) When a bar of brass was placed so as to vibrate on two parallel upright plates, also of brass, respectively connected with the poles of a battery, the

* From the Edinburgh New Philosophical Journal for April 1859.

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vibrations continued when the whole was immersed partly or wholly in water, and even when *flooded* by a powerful continued stream of cold water from a $\frac{3}{8}$-inch pipe under considerable pressure. From this experiment I conclude that the effect of the heat developed by the electrical current in the thin upright plates may be fairly considered to be reduced so low as to be incapable of producing a sensible result (if such were ever the case). Indeed, allowing for the resistance and friction of the water tending to diminish the vibration, there is no ground for thinking that the action was less energetic in the one case than in the other. It is consequently reasonable to conclude that the effect in question is due to the repulsive action of the electricity in passing from one conducting body to another, and not to its effect in producing expansion.

Now this is precisely the effect which I attributed to heat in the paper of 1833 already referred to. I therefore consider it a strong confirmation of the opinion I then expressed, from which I have never swerved, although it has not in general been received with much favour. The importance which I attach to this new confirmation, and the suggestiveness of Mr. Gore's experiment on the rolling-ball, will be judged of from the fact, that in 1833, or earlier, I had an apparatus made consisting of a bar resembling Mr. Trevelyan's, but longitudinally divided by a non-conducting partition; while the two conducting sides were furnished with mercury cups for connecting them with the poles of a battery, the circuit being completed through the metallic base. The instrument exists, or existed a few years ago, though I am at present unable to find it. As well as I recollect, it was tried with an old-fashioned Cruickshank's battery of fifty pairs, without success. Indeed I now find that, even with modern appliances, the experiment does not succeed when the circuit is only closed whilst *both* points of bearing of the rocker touch the mass or support.

December 20, 1858.

Since the date of the preceding notice (which was prepared for being laid on the table of the Royal Society at their meeting on the 20th ult.), I have continued and extended these experiments. As they are still in progress, I will content myself with mentioning two results as worthy of notice. I have obtained very active vibrations of carbon (such as is used in one of the elements of Bunsen's battery) resting upon brass, and also when it rests upon two pieces of carbon connected with the terminals of a battery. For this purpose, a battery having a certain amount of intensity is requisite, in order to overcome the resistance of carbon as a conductor; but the vibrations are most ener-

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getic. The extremely small expansion which takes place in carbon by heat is another argument against that view of the Trevelyan experiment. The other experiment to which I refer, is, that bismuth (and perhaps other metals) are not merely inactive as vibrators with any electric power which I have used, but the passage of electricity through them appears to have a *quelling* power which brings the rocker to instantaneous rest; yet bismuth permits a far freer passage of electricity than carbon: in one experiment I found that sixteen times as much was conducted. Something analogous was formerly observed by me in connexion with heat applied to bismuth. I am now attempting to investigate the subject further by experiment.

January 3, 1859.

LX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 305.]

November 25, 1858.—W. R. Grove, Esq., V.P., in the Chair.

THE following communications were read:—

"Researches on the Phosphorus-Bases."—No. III. Phosphoretted Ureas. By A. W. Hofmann, Ph.D., F.R.S.

The existence in the nitrogen-series of a well-defined group of diamines of the formula

$$\left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} N_2,$$

rendered it probable that the continued study of the phosphorus-, arsenic-, and antimony-bases would lead to the discovery of the corresponding terms,

$$\left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} P_2, \quad \left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} As_2 \text{ and } \left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} Sb_2,$$

which might be designated as diphosphines, diarsines, and distibines; and the further prosecution of this line of thought very naturally suggested the idea of searching for a group of intermediate compounds containing simultaneously nitrogen and phosphorus, nitrogen and arsenic, phosphorus and arsenic, &c., compounds expressed by the formulæ

$$\left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} NP; \quad \left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} N\,As; \quad \left.\begin{matrix} A_2 \\ B_2 \\ C_2 \end{matrix}\right\} P\,As, \text{ \&c.},$$

which might be termed phosphamines, arsamines, phospharsines, &c.

Among the several processes likely to furnish this result, none appeared more promising than the reaction between a monamine and a monophosphine of opposite chemical characters.

In the conception of this idea, I have studied the deportment of

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cyanic acid and some of its derivatives with phosphoretted hydrogen and its homologues, in the hope of producing combinations similar in constitution to the ureas, but differing from these substances by containing phosphorus in the place of one equivalent of nitrogen.

The action of cyanate and sulphocyanide of phenyle, an account of which I have lately* submitted to the Royal Society, upon triethylphosphine, seemed to include the conditions for the realization of such compounds.

On bringing cyanate of phenyle in contact with triethylphosphine, a most lively reaction ensues; the mixture begins to boil, and the phosphorus-base is apt to be inflamed. On cooling, the liquid solidifies into a crystalline mass, which is insoluble in water, soluble in alcohol and ether, and crystallizes from the latter solvent in beautiful little square tables, tasteless, inodorous, and infusible at 100° C. On submitting this compound to analysis, I was surprised to find that it contained no phosphorus, and that it exhibited the composition of the original cyanate of phenyle, from which it differs so essentially in its properties. This substance is the *cyanurate of phenyle*, generated from the cyanate by simple transposition of the elements. The triethylphosphine participates only indirectly in the reaction; in giving rise to the transformation of the cyanate, the phosphorous body plays the part of a ferment, a comparison which is moreover suggested by the large proportion of cyanate over which the influence of a minute quantity of phosphorus-base extends. A glass rod moistened with triethylphosphine solidifies, almost instantaneously, a considerable quantity of the cyanate. The transformation of the cyanate under the influence of triethylphosphine, into cyanurate, although the principal phase of the reaction, is attended by other changes which I intend to examine more minutely by and by.

Very different results were obtained by substituting for the cyanate the sulphocyanide of phenyle. The reaction between this body and triethylphosphine is very violent, and frequently gives rise to the inflammation of the phosphorus-base. The mixture assumes a deep yellow colour, and often deposits splendid yellow needles on cooling; frequently, however, it remains liquid for hours and even for days, but suddenly solidifies, when touched with a glass rod, into a hard, yellow, crystalline mass. This substance is insoluble in water; it dissolves with the greatest facility in alcohol, hot or cold, likewise in warm, less so in cold ether. Recrystallization from boiling ether affords, in fact, the best means of procuring the new body in a state of purity. This end is likewise considerably facilitated, by allowing the sulphocyanide of phenyle to act upon the triethylphosphine in the presence of a considerable quantity of ether.

In the pure state the new compound presents itself in the form of well-defined prisms of uranium-yellow colour, which fuse at 61° C. They cannot be heated much beyond their fusing-point without being altered; at 100° C. they are entirely decomposed, evolving a most peculiar odour, which is also observed on evaporating the ethereal mother-liquor.

* Proceedings, vol. ix. p. 274.

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The new compound possesses the characters of a well-defined base. Quite insoluble in water, it dissolves in the most dilute acids, forming with some of them, such as hydrochloric and hydrobromic acid, beautifully crystallized saline compounds. From these salts the base may be separated again by cautiously adding either potassa or ammonia. The hydrochloric solution of the base yields with dichloride of platinum a yellow crystalline precipitate, sparingly soluble in water, insoluble in alcohol and ether.

Analysis of the yellow crystals, dried over sulphuric acid, led to the formula

$$C_{26}H_{20}NPS_2,$$

which shows that they are formed by the simple union of the two substances placed in contact:

$$\underbrace{C_{14}H_5NS_2}_{\text{Sulphocyanide of phenyle.}} + \underbrace{C_{12}H_{15}P}_{\text{Triethyl-phosphine.}} = \underbrace{C_{26}H_{20}NPS_2}_{\text{New compound.}}.$$

If we consider urea as a diamine derived from diammonia by the substitution of the diatomic molecule carbonyle $(C_2O_2)''$ for 2 equivs. of hydrogen,

$$\text{Urea } C_2H_4N_2O_2 = \left.\begin{matrix}(C_2O_2)''\\H_2\\H_2\end{matrix}\right\} N_2,$$

—the simplest perhaps of the many views brought forward regarding the constitution of urea,—the new substance, which formation as well as chemical deportment essentially characterize as an analogue of urea, may be represented by the following formula:—

$$C_{26}H_{20}NPS_2 = \left.\begin{matrix}(C_2S_2)''\\(C_4H_5)_2\\(C_{12}H_5)(C_4H_5)\end{matrix}\right\} NP;$$

that is, urea, the oxygen of which is replaced by sulphur, the hydrogen by ethyle and phenyle, and lastly, half the nitrogen by phosphorus.

The formation of this compound presents considerable interest, not only as an illustration of the remarkable persistence of the type urea, but also as furnishing the first unequivocal instance of the formation of ureas containing no longer any unreplaced hydrogen, the existence of which had as yet remained doubtful.

The new urea forms, as I have stated, a series of well-defined beautifully crystallized salts. Its solution in warm hydrochloric acid solidifies, on cooling, into a crystalline mass, which, when recrystallized from warm water, is obtained in splendid needles of a bright cadmium-yellow colour, often several inches in length. They are decomposed at 100° C., and must therefore be dried over sulphuric acid *in vacuo*. Analysis proved them to contain

$$C_{26}H_{20}NPS_2, HCl.$$

The solution of this salt yields with dichloride of platinum a bright yellow precipitate, which under the microscope is found to consist of small lily-shaped crystals. Dried over sulphuric acid *in vacuo* it contains

$$C_{26}H_{20}NPS_2, HCl, PtCl_2.$$

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The hydrochlorate yields also a precipitate with trichloride of gold; the salt is, however, rapidly blackened.

The hydrobromate, both in preparation and properties, resembles the hydrochlorate. Its composition is

$$C_{26}H_{20}NPS_2, HBr.$$

The urea readily combines with iodide of methyle and ethyle. The methyle-compound immediately separates in the crystalline form on mixing an ethereal solution of the urea with iodide of methyle; it is soluble in water, and crystallizes from a boiling solution in beautiful golden-yellow needles, containing

$$C_{26}H_{20}NPS_2, C_2H_3I.$$

The iodide, by the action of chloride of silver, may be converted into the chloride; this yields with dichloride of platinum a fine needle-formed salt, which may be recrystallized without decomposition. The formula of this platinum-salt is

$$C_{26}H_{20}NPS_2, C_2H_3Cl, Pt, Cl_2.$$

When treated with oxide of silver, the iodide furnishes a powerfully alkaline liquid, probably the corresponding base

$$\left.\begin{matrix}[(C_2H_3)((C_2S_2)''(C_4H_5)_3(C_{12}H_5)\underset{H}{N}P)]\end{matrix}\right\}O_2.$$

Scarcely separated, however, this substance decomposes with liberation of sulphocyanide of phenyle, the oxide of methyl-triethylphosphonium remaining in solution. This salt is sufficiently characterized by the readily crystallizable octahedral platinum-salt.

I have not been able to obtain either the sulphate or the nitrate of the urea, probably on account of the great instability of the new substance.

On dissolving the base, even in dilute nitric acid, it is immediately decomposed with separation of sulphocyanide of phenyle, the triethylphosphine being oxidized. The same change is observed when one of the more stable salts, such as the hydrochlorate, is dissolved in a large quantity of water; the liquid soon becomes turbid from the elimination of oily globules of sulphocyanide of phenyle, and now contains the hydrochlorate of the phosphorus-base.

On adding ammonia to a salt of the urea, similar phenomena are observed. From a concentrated solution, the base is separated without change; but when dilute and hot solutions are employed, the turbidity at first produced disappears, and after a few minutes beautiful crystals of phenyl-sulphocarbamide ($C_{14}H_8N_2S_2$)* make their appearance; at the same time the odour of triethylphosphine becomes perceptible.

With potassa the deportment is perfectly analogous, but the crystals formed after some time are diphenyl-sulphocarbamide (sulphocarbanilide, $C_{26}H_{12}N_2S_2$) instead of phenyl-sulphocarbamide.

On adding to an ethereal solution of the urea a few drops of bisulphide of carbon, the liquid, when gently heated, assumes a deep crimson colour, and deposits, on cooling, the beautiful compound $(C_4H_5)_3P, C_2S_4$, which I have described some time ago†. The

* Proceedings of the Royal Society, vol. ix. p. 276. † Ibid. p. 290.

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mother-liquor yields on evaporation oily drops of sulphocyanide of phenyle.

The deportment of triethylphosphine with sulphocyanide of phenyle induced me to investigate the action of this body upon several other sulphocyanides. The substance which at once suggested itself for examination was sulphocyanide of allyle, mustard-oil. This compound reacts most powerfully with the phosphorus-base. On mixing the two bodies, a powerful evolution of heat takes place, and the mixture assumes a deep brown colour, but does not solidify either on cooling or on agitation. After several days' standing, however, very large well-defined crystals are deposited which unfortunately are contaminated with the brown colouring matter of the solution. I have not yet succeeded in getting them perfectly white, and have therefore not analysed them. Their formation, however, and their general characters leave no doubt that they are the corresponding allyle-compound,

$$C_{20}H_{20}NPS_2 = \left.\begin{matrix}(C_2S_2)'' \\ (C_4H_5)_2 \\ (C_4H_5)(C_6H_5)\end{matrix}\right\} NP.$$

Triethylphosphine has remained in contact with sulphocyanide of ethyle for more than a month without depositing any crystals. *A priori*, however, the formation of an urea under these circumstances was doubtful, since sulphocyanide of ethyle differs from the corresponding phenyle- and allyle-compounds, even in its deportment with ammonia and the monamines.

In concusion, it deserves to be mentioned that there appears to exist a similar series of arseniuretted ureas. Triethylarsine, when left for some weeks in contact with sulphocyanide of phenyle, deposits small crystals of a body which I believe to be the arsenic-compound corresponding to the phosphorus-urea described in this paper. This body requires a more minute examination.

"On the Deflection of the Plumb-line in India caused by the Attraction of the Himalaya Mountains." By the Venerable Archdeacon Pratt.

"Note on Archdeacon's Pratt's paper on the Effect of Local Attraction on the English Arc." By Captain Clarke, R.E.

"On the Thermal Effects of Compressing Fluids." By J. P. Joule, LL.D., F.R.S.

The author in this paper gives an experimental demonstration of the correctness of Professor Thomson's formula, $\theta = \frac{Tep}{JK}$, where θ is the thermal effect, T the temperature from absolute zero, e the expansibility by heat, p the pressure, J the mechanical equivalent of the thermal unit, and K the capacity for heat. The fluids experimented on were water and oil, with the results tabulated below:—

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	Temperature of the liquid.	Pressure applied in atmospheres.	Experimental result.	Theoretical result.
Water...	1°·2 Cent.	25·34	−0°·0083	−0°·0071
	5	25·34	0·0044	0·0027
	11·69	25·34	0·0205	0·0197
	18·38	25·34	0·0314	0·0340
	30	25·34	0·0544	0·0563
	31·37	15·64	0·0394	0·0353
	40·4	15·64	0·0450	0·0476
Oil	16	7·92	0·0792	0·0886
	17·29	15·64	0·1686	0·1758
	16·27	25·34	0·2663	0·2837

December 9.—Sir Benjamin Collins Brodie, Bart., President, in the Chair.

The following communications were read:—

"Researches into the Nature of the Involuntary Muscular Tissue of the Urinary Bladder." By George Viner Ellis, Professor of Anatomy in University College, London.

"On the Ova and Pseudova of Insects." By John Lubbock, Esq., F.R.S., F.L.S., F.G.S.

December 16.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

Extract of a Letter from Professor Lamont to Major-General Sabine, Treas. and V.P.R.S., dated Munich, Dec. 19, 1858.

"My magnetic observations in France, Spain, and Portugal are now published, and a copy for you is on the way. The observations of last summer are under the press. They comprehend about thirty stations in the North of Germany, Belgium, Holland, and Denmark. Next year I am going to Italy, and in 1860 I intend to revisit Spain, in order to observe the total eclipse of the sun, and also to make magnetic observations.

"I have found that on the Continent the lines of horizontal intensity move from south-west to north-east, making an angle of about 20° with the meridian, that is, in a direction coinciding with the lines of declination. The lines of inclination seem to move in the same direction, and the motion of the lines of declination will probably coincide with the lines of horizontal intensity. Calling $+\Delta H$ and $-\Delta i$ the annual changes of horizontal intensity and inclination of a central station (suppose London), the annual changes for a place situated x degrees in latitude to the north, and y degrees in longitude to the west, will be—

$\Delta H - 0{\cdot}00018x + 0{\cdot}00008y$ (absolute measure, French units),

$-\Delta i + 0'{\cdot}21x \quad -0'{\cdot}09y$.

"The new survey of the British Islands will offer an opportunity of testing the correctness of these formulæ."

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"Description of a mutilated skull of a large Marsupial Carnivore (*Thylacoleo Carnifex*, Ow.), from a conglomerate stratum, eighty miles S.W. of Melbourne, Australia." By Professor R. Owen, F.R.S. &c.

"On the Nature of the Action of Fired Gunpowder." By Lynall Thomas, Esq.

Since the year 1797, when Count Rumford made his experiments for ascertaining the initial force of fired gunpowder, an account of which appears in the Philosophical Transactions of that year, very little light has been thrown on the subject. Count Rumford's experiments, valuable in many respects, afforded indeed nothing conclusive respecting it.

The object of the present paper is to show the unsatisfactory nature of the present theory of the action of gunpowder, and to point out some of the principal errors upon which this theory is based. For this purpose, the results of various experiments made by the author, and which were repeated in the presence of a Select Committee at Woolwich, are described and explained.

These experiments are held by the author not only to afford complete evidence of the unsoundness of the present theory, but as sufficiently conclusive to serve as a basis for the formation of a new set of formulæ, both correct and simple, in place of those at present in use.

The initial action of the fired charge of powder upon the shot,—the first movement of the shot itself in the gun,—and the force exerted upon the gun by different charges of powder,—and, therefore, the actual strength of metal required for the gun,—are circumstances, which, as the author believes, have not only been misunderstood, but for which laws have been assigned directly opposed to the truth.

As an instance of this, the hitherto received theory supposes that when a shot is fired from a gun, it acquires its velocity gradually, from the pressure of the elastic fluid generated by the fired powder acting upon it through a certain space. It is also supposed that the initial pressure of this elastic fluid is the *same* in all cases (the quantities of powder being proportional), whether the gun from which the shot is fired be large or small; so that the larger the calibre of the gun, the slower the first movement of the shot is supposed to be. The result of the following experiment is given to prove that the first of these propositions is incorrect. The author placed a cast-iron shot 3 inches in diameter and 3 lbs. 14 ozs. in weight upon a chamber half an inch in diameter and half an inch deep. This chamber was formed in a block of gun-metal, and contained, when filled, one dram of powder. Upon lighting the powder, the ball was driven to a height of 5 feet 6 inches; when the ball was placed at $\frac{1}{8}$ of an inch over the chamber, the charge failed to move it.

From this it is inferred that the first force of the powder is an *impulsive* force, that is to say, it imparts to the shot at once a finite velocity. In order to place the matter beyond a doubt, and to ascertain the relative force of different quantities of powder, the author caused a

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chamber to be made similar in form to, but of twice the linear dimensions of, the former; he then placed a cast-iron ball of 6 inches in diameter upon the orifice of this chamber, which was filled with powder; upon firing the latter, the ball was driven up to a height of 11 feet, that is to say, to double the height of the smaller; the state of the metal in which the chamber was formed also showed the increase in the initial force of the powder: this is considered to be sufficient proof that the last two of the above-mentioned propositions are as incorrect as the first.

Assuming the initial force of the powder to be of an impulsive nature, it is not difficult to understand the increase of force shown in the last-named experiment, inasmuch as a certain time being required for the complete conversion of the powder into an elastic fluid, a quantity contained in a chamber of a similar form, but of greater linear dimensions than another, must ignite in a less comparative time, the linear dimensions increasing in the ratio of the first power, and the quantity of powder increasing in the ratio of the third power, so that the flame will traverse a larger quantity in comparative less time.

Thus it appears that the powder which inflames more rapidly has a much greater initial force, being more *concentrated* in its action; a quick burning powder therefore is better for ordnance of small length, such as mortars and iron howitzers. The different results produced by powder of different quality have, according to the author, been entirely overlooked in the hitherto received theory. This theory, which considers the *secondary* force, namely, the elasticity of the fluid only, and takes no account whatever of the enormous impulsive, or initial force, produced by the sudden conversion of the powder into an elastic fluid, is that which regulates the system upon which ordnance are at present constructed; hence the reason why large guns are so liable to burst, so much so, that it has been said that no gun larger than a 32-pounder is safe to fire. From the variety of experiments made by the author, he arrives at the conclusion, that when powder is of the same quality, and confined in chambers of similar form, but of different sizes, the initial force varies, within certain limits, in the ratio of $\frac{w^{\frac{4}{3}}}{w'}$, where w is the weight of the powder and w' of the ball.

Thus were this new theory recognized, the question of the increase of strength with increased thickness of metal, would wear an entirely new aspect. So far from the metal in large guns diminishing in strength in the proportion assumed, it will be a matter for inquiry how it resists the great strain to which it is subjected, rather than why it yields; for we find from the experiments described above, that a 68-pounder gun, which has a calibre of twice the diameter of a 9-pounder gun, must, when fired with the same proportionate charge of powder as the latter, continually be subject to as great a strain as the latter would suffer if always fired with the proof charge, which is three times the quantity of the ordinary service charge.

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January 6, 1859.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

Letter to Dr. Sharpey, Sec. R.S., from Dr. Thomas Williams, F.R.S., dated Swansea, Dec. 12, 1858.

"A Sixth Memoir on Quantics." By Arthur Cayley, Esq., F.R.S.

"Contributions towards the History of the Monamines." By A. W. Hofmann, LL.D., F.R.S.

2. *Action of Bisulphide of Carbon upon Amylamine.*

In a note on the alleged transformation of thialdine into leucine, addressed to the Royal Society about eighteen months ago*, I alluded to a crystalline substance observed by Wagner when submitting amylamine to the action of bisulphide of carbon. This substance was not analysed, but considering its mode of formation, Wagner suggested that it might possibly be thialdine.

$$\underbrace{C_{10}H_{13}N}_{\text{Amylamine.}} + C_2S_4 = \underbrace{C_{12}H_{13}NS_4}_{\text{Thialdine.}}.$$

A superficial comparison of the properties of thialdine with those of the substance produced by the action of bisulphide of carbon upon amylamine, enabled me at once to recognize the difference of the two bodies; and satisfied with the result, I did not at the time examine more minutely into the nature of the latter substance.

The new interest conferred upon leucine by recent researches which characterize this substance as capronamic acid, has called my attention back to the sulphuretted derivative of amylamine.

This body may be readily procured by mixing anhydrous amylamine with a solution of dry bisulphide of carbon in anhydrous ether. The mixture becomes hot, and deposits, on cooling, white shiny scales which are scarcely soluble in ether, and may therefore be purified by washing with this liquid.

The new body is likewise insoluble in water, but readily dissolves in alcohol; when dry, it may be exposed for a time to a temperature of 100° C. without undergoing fusion; after some time, however, the substance begins to be liquefied and to undergo complete decomposition. The same change occurs, although more slowly, at the common temperature, when sulphuretted hydrogen is evolved; a mixture of free sulphur with a new crystalline substance, extremely fusible, insoluble in water, but soluble both in alcohol and ether, remaining behind.

Analysis has proved that the compound produced by the action of bisulphide of carbon upon amylamine contains

$$C_{11}H_{13}NS_2, \text{ or rather } C_{22}H_{26}N_2S_4;$$

* Proceedings, vol. viii. Op. 4.

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and that it is formed by the union of 2 equivalents of amylamine with bisulphide of carbon.

$$\underbrace{2C_{10}H_{13}N}_{\text{Amylamine.}}+C_2S_4=\underbrace{C_{22}H_{26}N_2S_4}_{\text{New compound.}}$$

A glance at this formula suffices to characterize this compound as amylsulphocarbamate of amylamine.

$$C_{22}H_{26}N_2S_4=C_{10}H_{13}N,C_{12}H_{13}NS_4=\left.\begin{matrix}[(C_2S_2)''(C_{10}H_{11})HN]\\[(C_{10}H_{11})H_3N]\end{matrix}\right\}S_2.$$

This view is easily confirmed by experiment. Addition of hydrochloric acid to the crystalline compound immediately separates an oily liquid, which gradually solidifies, and the acid solution now contains amylamine which may be liberated by potassa. The oily substance is obviously amylsulphocarbamic acid: it dissolves in ammonia and in potassa; mixed with amylamine, it reproduces the original crystalline compound.

Experiments with ethylamine have furnished perfectly analogous results. I have been satisfied to establish qualitatively the analogy of the reactions.

It is of some interest to compare the deportment of amylamine under the influence of bisulphide of carbon with that of phenylamine in the same conditions. If both bodies gave rise to similar changes, we should expect in the case of phenylamine the formation of phenylsulphocarbamate of phenylamine. But experiment has proved that phenylamine immediately produces diphenyl-sulphocarbamide (sulphocarbanilide), sulphuretted hydrogen being evolved—

$$\underbrace{2(C_{12}H_7N)}_{\text{Phenylamine.}}+C_2S_4=\underbrace{C_{26}H_{12}N_2S_2}_{\text{Diphenylsulpho-carbamide.}}+H_2S_2.$$

Nevertheless it is extremely probable that further experiments will establish a perfect analogy in the deportment of bisulphide of carbon with amylamine and phenylamine. Diphenyl-sulphocarbamide is probably the product of decomposition of a very unstable phenylsulphocarbamate of phenylamine—

$$\underbrace{C_{26}H_{14}N_2S_4}_{\text{Phenylsulpho-carbamate of phenylamine?}}=H_2S_2+\underbrace{C_{26}H_{12}N_2S_2}_{\text{Diphenylsulpho-carbamide.}},$$

while a more minute examination of the crystalline substance obtained by the action of heat upon amylsulphocarbamate of amylamine cannot fail to characterize it as diamylsulphocarbamide—

$$\underbrace{C_{22}H_{26}N_2S_4}_{\text{Amylsulphocarbi-mate of amylamine.}}=H_2S_2+\underbrace{C_{22}H_{24}N_2S_2}_{\text{Diamylsulpho-carbamide.}}?$$

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The apparent dissimilarity of the two reactions would thus be reduced to the unequal stability of the sulphocarbamic acids of the amyle- and phenyle-series.

"On New Nitrogenous Derivatives of the Phenyle- and Benzoyle-series." By P. Griess, Esq.

Piria's important discovery that the action of nitrous acid upon asparagine gives rise to the formation of malic acid, has led to a very general application of this agent in the study of nitrogenous substances. The results obtained have been almost always analogous to those produced by Piria; the reaction may be illustrated by the following examples:—

$$\underbrace{\left.\begin{matrix}(C_8H_4O_6)''\\H_4\end{matrix}\right\}}_{\text{Asparagine.}}N+2NO_3=H_2O_2+4N+\underbrace{\left.\begin{matrix}(C_8H_4O_6)''\\H_2\end{matrix}\right\}}_{\text{Malic acid.}}O_4.$$

$$\underbrace{\left.\begin{matrix}C_{12}H_5\\H_2\end{matrix}\right\}}_{\text{Phenylamine.}}N+NO_3=HO+2N+\underbrace{\left.\begin{matrix}C_{12}H_5\\H\end{matrix}\right\}}_{\text{Phenol.}}O_2.$$

The plan hitherto adopted consisted in submitting the *aqueous* solution of the nitrogenous body directly to the action of nitrous acid, or in dissolving the body in nitric acid, and passing into the solution a current of binoxide of nitrogen. By employing *alcoholic* and *ethereal* solutions, I have arrived at different results, establishing a *new mode of reaction*; of the facts which I have observed the following may be quoted as illustrations.

Action of Nitrous Acid on Picramic Acid. Diazodinitrophenol.

On passing a current of nitrous acid into an alcoholic solution of picramic acid—

$$C_{12}H_5N_3O_{10}=C_{12}\left(\begin{matrix}H_3\\(NO_4)_2\\H_2N\end{matrix}\right)O_2,$$

the red liquid assumes at once a yellow colour, and furnishes rapidly a copious deposit of yellow crystals. *No gas is evolved during the reaction.* The yellow crystals, purified by recrystallization from alcohol, are found to contain

$$C_{12}H_2N_4O_{10},$$

and are obviously formed according to the equation—

$$C_{12}H_5N_3O_{10}+NO_3=3HO+C_{12}H_2N_4O_{10}.$$

The new body, for which I propose the provisional name *diazodinitrophenol*, is soluble in alcohol and ether, and crystallizes from the former solvent in magnificent golden-yellow plates, which detonate on heating. Acids have no action upon this substance; on ebullition with water it appears to undergo decomposition; alkalies induce at once a copious evolution of gas, and give rise to the formation of dinitrophenol. This metamorphosis appears to indicate that the new body still belongs directly to the phenol-group; the constitu-

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tion of diazodinitrophenol may perhaps be best understood by representing it by the formula

$$C_{12}\left(\begin{matrix}H_2\\(NO_4)_2\\N_2\end{matrix}\right)O_2.$$

The transformation of this compound into

$$C_{12}\left(\begin{matrix}H_4\\(NO_4)_2\end{matrix}\right)O_2$$

involves the decomposition of 2 equivs. of water, the oxygen of which appears to be consumed in the formation of secondary products of decomposition. No trace of oxygen, either free or combined, could be found among the gaseous products; the gas evolved consisting, according to a minute examination, of perfectly pure nitrogen.

Diazonitrochlorphenol.

Treatment in a similar manner of *amidonitrochlorphenol*

$$C_{12}H_4ClNO_6=C_{12}\left(\begin{matrix}H_2\\Cl\\NO_4\\H_2N\end{matrix}\right)O_2,$$

a new mixed derivative of phenol, as might have been expected, has furnished perfectly similar results. The new compound thus obtained crystallizes in beautiful brown-red needles, of physical and chemical properties similar to those of the preceding compound. It contains

$$C_{12}H_2ClN_3O_6=C_{12}\left(\begin{matrix}H_2\\Cl\\NO_4\\N_2\end{matrix}\right)O_2.$$

Diazonitrophenol.

This substance is formed by submitting the ethereal solution of *diphenamic acid*,

$$C_{24}H_{12}N_4O_{12}=C_{24}\left(\begin{matrix}H_8\\(NO_4)_2\\(H_2N)_2\end{matrix}\right)O_4,$$

discovered by Gerhardt and Laurent, to the action of nitrous acid. It is a yellow, crystalline, very unstable compound, containing

$$C_{24}H_6N_6O_{12}=C_{24}\left(\begin{matrix}H_6\\(NO_4)_2\\N_4\end{matrix}\right)O_4;$$

it explodes with extreme violence at the temperature of boiling water. The alkalies decompose it instantaneously with evolution of nitrogen and formation of products which are not yet analysed.

Action of Nitrous Acid upon Benzamic Acid.

The product obtained in a similar manner from *benzamic acid* is an orange-yellow crystalline precipitate, which constitutes a dibasic

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acid of the formula

$$C_{24}H_{11}N_3O_8.$$

Its formation is illustrated by the following equation:

$$\underbrace{C_{24}H_{14}N_2O_8}_{\text{2 equivs. of benzamic acid.}}+NO_3=3HO+\underbrace{C_{24}H_{11}N_3O_8}_{\text{New acid.}}.$$

This acid is insoluble in water, alcohol, and ether. It is dissolved without decomposition by the alkalies in the cold, giving rise to the formation of soluble crystalline salts, which produce precipitates with nitrate of silver and acetate of lead.

All these salts are decomposed on heating, with evolution of nitrogen gas. The action of fuming nitric acid upon the dibasic derivative of benzamic acid produces a new acid, furnishing with barium a splendid yellow crystalline salt. The dibasic acid is likewise decomposed by hydrochloric acid; in combination with this acid remains a body which can be sublimed in white crystals.

An alcoholic solution of benzamic ether when treated with nitrous acid yields the ether of the acid previously described.

The action of nitrous acid on alcoholic solutions of cuminamic and anisamic acids has likewise furnished new bodies, with the study of which I am at present engaged.

Action of Nitrous Acid on Phenylamine and Nitrophenylamine.

Phenylamine, when submitted to the modified nitrous acid-process, is transformed into a fusible body containing

$$C_{24}H_{11}N_3,$$

which is insoluble in water and easily soluble in alcohol. This compound, which possesses feebly basic characters, is formed according to the equation

$$\underbrace{C_{24}H_{14}N_2}_{\text{2 equivs. of Phenylamine.}}+NO_3=3HO+\underbrace{C_{24}H_{11}N_3}_{\text{New compound.}}.$$

Nitrophenylamine (the alpha-variety which is formed by the action of reducing agents upon dinitrobenzol), similarly treated, furnishes a compound crystallizing in beautifully red needles,

$$C_{24}H_9N_5O_8,$$

the formation of which is represented by the equation

$$\underbrace{C_{24}H_{12}N_4O_8}_{\text{2 equivs. of Nitrophenylamine.}}+NO_3=3HO+\underbrace{C_{24}H_9N_5O_8}_{\text{New compound.}}.$$

Treated with concentrated hydrochloric acid, the new compound reproduces nitrophenylamine. The action of chlorine and bromine upon it gives rise to the formation of new crystallized derivatives.

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GEOLOGICAL SOCIETY.

[Continued from p. 310.]

March 23, 1859.—Prof. J. Phillips, President, in the Chair.

The following communications were read:—

1. "On some Amphibian and Reptilian Remains from South Africa and Australia." By Thomas H. Huxley, F.R.S., Sec. G.S., Prof. of Natural History, Government School of Mines.

The author described in the first place the remains of a small Labyrinthodont Amphibian, which he proposed to call *Micropholis Stowii*. The fossil was discovered by Mr. Stow, and accompanied that gentleman's paper "On some Fossils from South Africa," read before the Society on the 17th of November last, on which occasion Prof. Huxley expressed the opinion that it would prove to be an Amphibian, and probably a Labyrinthodont.

It had been found impossible to work out the back part of the skull, so as to exhibit the occipital condyles; but the characters of the few cranial bones which remain, of the teeth, and of the lower jaw, and the traces of a largely developed hyoidean apparatus, afforded sufficiently convincing evidence of the affinities of *Micropholis*.

The generic appellation is based on the occurrence of numerous minute polygonal bony scutes on the integument of the under surface of the head; in which character *Micropholis* has a remote resemblance to *Archegosaurus*. The scutes, however, are very different in their aspect from those of the last-named genus.

Micropholis has little resemblance with any European Labyrinthodonts, except *Metopias*, and the singular so-called "*Labyrinthodon Bucklandi*," from the Trias of Warwickshire, to the peculiarities of which the author alluded, proposing to consider it as the type of a new genus, which might be termed "*Dasyceps*."

On the other hand, there are two southern forms of Labyrinthodont, which exhibit many similarities to *Micropholis*. These are the *Brachyops laticeps* of Prof. Owen, from Central India, and a new form allied to *Brachyops*, but distinct from it, from Australia. This last was described and named *Bothriceps australis*.

The author stated that he was not prepared to draw any very decided conclusion, as to the age of the Karoo- or Dicynodon-beds, from the fact of the occurrence of Labyrinthodont *Amphibia* in them, inasmuch as the Labyrinthodonts range from the Lower Lias to the Carboniferous Formation inclusive; and *Micropholis* is unlike any of the Labyrinthodonts whose precise age is known.

The fragmentary remains of a young reptile, which were found associated with *Micropholis*, were stated by Prof. Huxley to be undoubtedly those of a *Dicynodon*. Of this, however, and of a small Dicynodont skull from the same locality, he promised to speak on a future occasion.

The second part of the paper consisted of a description of the structure of the cranium, of the sclerotic ring, of a fragmentary

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sacrum, and of the humerus of the new species of *Dicynodon* (*D. Murrayi*) from near Colesberg, which was characterized at a previous meeting of the Society (February 23). Particular attention was directed to the unusually complete ossification of the cranio-facial axis, and to the striking resemblance in the structure of the bony walls of the olfactory apparatus to that which obtains in Birds. Prof. Huxley, in conclusion, gave a sketch of the general proportions of the *Dicynodon*, so far as the evidence as yet obtained allows a judgment to be formed, and particularly alluded to the existence of a long series of caudal vertebræ. Specimens of the fossil wood found with the remains of *D. Murrayi* had been submitted to Dr. Hooker, and declared by him to be coniferous.

2. "On *Rhamphorhynchus Bucklandi*, a Pterosaurian from the Stonesfield Slate." By Thomas H. Huxley, F.R.S., Sec. G.S., Prof. of Natural History, Government School of Mines.

The author based his account of this Pterosaurian upon a fine fragment of a lower jaw, discovered by the Earl of Ducie in the quarries of Sarsden, near Chipping Norton,—on a coracoid bone from the Stonesfield slate, in the collection of the Museum of Practical Geology,—on a large fragment of a lower jaw in the Museum of the Society, and a very fine specimen of a lower jaw in the Museum of the College of Surgeons. The ascription of the coracoid to the same species as that to which the jaws belong was admitted to be hypothetical; but their proportions agree sufficiently well to give probability to the supposition. Furthermore, the author did not suppose it to be absolutely demonstrable that the jaws and coracoid in question, supposing them to be of one species, were of the same species as those Pterosaurian remains discovered by Dr. Buckland in the Stonesfield slate many years ago, and (though never described) named after him *Pterodactylus Bucklandi*; but, as a specific name unaccompanied by a description is of no authority, and as there is no evidence of the existence of more than one species of Pterosaurian in the Stonesfield slate, it seemed that the adoption of the specific name *Bucklandi* would have the least tendency to create confusion.

These remains prove that the Stonesfield Pterosaurian belonged to the genus *Rhamphorhynchus* of Von Meyer, and that it had nearly twice the size of the liassic *Dimorphodon macronyx*. The mandible of *R. Bucklandi* is remarkable for its stoutness and the depth of its rami towards the symphysis, which is short and produced into a stout, curved, median, edentulous rostrum. The teeth are similar in form, flattened and sharp-pointed, distinct, and not more than seven in number on each side: the last tooth is situated rather behind the junction of the middle with the posterior third of the jaw. The author took occasion to refer incidentally to some undescribed peculiarities in the structure of the coracoid of *Dimorphodon macronyx*.

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3. "On a Fossil Bird and a Fossil Cetacean from New Zealand." By Thomas H. Huxley, F.R.S., Sec. G.S., Prof. of Natural History, Government School of Mines.

These remains were, the right tarso-metatarsal bone of a member of the Penguin family, allied to *Eudyptes*, but indicating a bird of much larger size than any living species of that genus, larger indeed than even the largest *Aptenodytes*, and to which the name of *Palæudyptes antarcticus* was given,—and the left humerus of a small cetacean, more nearly resembling that of the common Porpoise than that of any other member of the order (*Balæna, Balænoptera, Monodon, Delphinus, Orca, Hyperoodon*) with which the author had been able to compare it. Nevertheless, as there are very marked differences between the fossil humerus and that of *Phocæna*, Prof. Huxley named the species *Phocænopsis Mantelli*. Mr. W. Mantell, F.G.S., to whom the author was indebted for the opportunity of examining these bones, stated that the beds whence they were obtained were certainly of Tertiary age, and of much earlier date than the epoch of the *Dinornis*, which he considered to have been contemporaneous with man. The *Palæudyptes* was from an older bed than the *Phocænopsis*.

Prof. Huxley drew attention to the remarkable fact that a genus so closely allied to the Penguins which now inhabit New Zealand, and are entirely confined to the Southern Hemisphere, should have existed at so remote an epoch in the same locality.

4. "On the Dermal Armour of *Crocodilus Hastingsiæ*." By Thomas H. Huxley, F.R.S., Sec. G.S., Prof. of Natural History, Government School of Mines.

The author, after briefly mentioning the very complete armour of articulated dorsal and ventral scutes which he had recently discovered (and described before the Linnean Society) in two of the three living genera of *Alligatoridæ*, viz. *Caiman* and *Jacare*, showed that similar scutes are found associated with the remains of *Crocodilus Hastingsiæ*, a very fine skull and some scutes of which reptile, from Hordwell, kindly lent to Prof. Huxley by Mr. S. Laing, F.G.S., were exhibited. With respect to the suggestion of Prof. Owen, that the *Alligator Hantoniensis* might possibly be a variety of *Crocodilus Hastingsii*, the author stated that he had observed in several specimens of the recent *Crocodilus palustris*, which by its straight premaxillo-maxillary suture and the general form of its skull most nearly approaches *C. Hastingsiæ*, a tendency to assume the alligator character of a pit, instead of a groove, for the reception of the mandibular canine. Sometimes there is a pit on one side and a groove on the other, and sometimes incomplete pits on both sides in this Crocodile. *Crocodilus Hastingsiæ* still more nearly approaches the *Alligatoridæ* in the number of its teeth and in the characters of the dermal armour now described; so that the probability of its occasionally assuming the Alligatorian dental pits on both sides is greatly increased.

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April 6, 1859.—Prof. J. Phillips, President, in the Chair.

The following communication was read:—

"On the Subdivisions of the Inferior Oolite in the South of England, compared with the Equivalent beds of the same formation on the Yorkshire Coast." By Thomas Wright, M.D., F.R.S.E. (Communicated by T. H. Huxley, Esq., Sec. G.S.) With a Note on Dundry Hill, by R. Etheridge, Esq., F.G.S.

The author first remarked that, since the publication of his memoir "On the so-called Sands of the Inferior Oolite" in the Society's Journal (vol. xii. p. 292), some geologists, both in England and on the Continent, had taken the Liassic character of these sands into consideration, and that Oppel, Hébert, Marcou, and Dewalque had agreed with the author on palæontological grounds, whilst in England Mr. E. Hull (of the Geological Survey) had also adopted his views. On the other hand, in recent memoirs, Mr. Lycett regards them as forming a distinct stage, and Prof. Buckman still retains them in the Inferior Oolite.

Dr. Wright then described the beds at Bluewick, on the Yorkshire coast, which he regards as the equivalents of the "Cephalopoda-bed" or "Jurensis-bed:" namely some shales and sandstones underlying the rock which he considers to be the basement-bed of the "Dogger" or Inferior Oolite.

These are—1. (uppermost) Shales with *Terebratula trilineata, Belemnites compressus, B. irregularis,* and *Trigonia Ramsayi*. 2. Sandstone, yellow, with *Turritella, Trigonia, Astarte, Ammonites concavus, A. variabilis,* &c. 3. Yellow Sandstone or Serpula-bed. 4. Grey Sandstone or Lingula-bed, with *Lingula Beanii, Orbicula, Belemnites compressus, B. irregularis, Ammonites Moorei,* &c.

The author then observed that the Inferior Oolite in the South of England admits of a palæontological subdivision into three zones, having the Fuller's Earth with *Ostrea acuminata* above, and the Cephalopoda-bed with *Ammonites opalinus* beneath:—1st (uppermost), the zone of *Ammonites Parkinsoni*; 2nd, zone of *Am. Humphriesianus*; and 3rd, zone of *Am. Murchisonæ*. He then described the lowest of these zones, that of *Am. Murchisonæ*, giving as synonyms "Dogger" (part), Young and Bird, and Phillips; "the Central and lower division of the Inferior Oolite," Murchison; "Fimbria-stage of the Inferior Oolite," Lycett; "Brauner Jura β," Quenstedt; "Calcaire lædonien" (part), Marcou; "Calcaire à entroques," Cotteau; "die Schichten des Am. Murchisonæ," Oppel. The Leckhampton section was then described, as illustrating this zone, which was also described in its details as seen at Crickley Hill, near Cheltenham, and at Beacon Hill; also at Frocester Hill and Wootton-under-Edge.

The preceding sections exhibit the lithological character and stratigraphical relations of the Pea-grit and Freestones, which, however, undergo great and very important modifications when examined over even a limited area,—the Pea-grit as regards its structure; and the Freestone, its thickness. In the Southern Cotteswolds the

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Pea-grit loses its pisolitic character; and in the eastern part of the hill-district the Freestones thin out and finally disappear; the Inferior Oolite being represented at Stow-on-the-Wold and at Burford by the zone of *Ammonites Parkinsoni*, with its light-coloured ragstones, filled with an abundance of *Clypeus Plotii*, Klein, and forming a "*Clypeus-grit.*"

The fossils of the Pea-grit and Freestone, and of the Oolite-marl or Fimbria-bed, were then enumerated. The Oolite-marl was described as having been probably derived from the debris of a Coral-reef: its Nerinæan limestone was particularly alluded to.

The section at the Peak near Robinhood's Bay afforded the author the equivalents of the zones of *Am. Humphriesianus* and *Am. Murchisonæ*, and was described in full.

The zone of *Am. Humphriesianus* was next treated of. Its synonyms are "Inferior Oolite of Dundry Hill," Conybeare and Phillips; "Grey limestone, Bath or Great Oolite" (Yorkshire), Phillips; "Eisenrogenstein (part) und Walk-Erde Gruppe," Fromherz; "Brauner Jura γ und δ," Quenstedt; "Calcaire ferrugineux," Terquem; "Blaue Kalke, Korallenschicht, Giganteus-Thone, und Ostreen-Kalke" (Quenstedt), Pfizenmayer. The best types of this zone, so well characterized by peculiar Gasteropods and Cephalopods and its ferruginous oolitic grains, are seen in the section at Dundry Hill, at Yeovil and Sherbourne in Somerset, and at Burton-Bradstock and Chideock in Dorset. Just as the thinning-out of the Murchisonæ-zone and the absence of the Humphriesianus-zone near Burford and other localities in the N.E. parts of the Northleach district brings the Parkinsoni-zone nearly into juxtaposition with the clays of the Upper Lias, so the thinning-out of the Murchisonæ-zone at Dundry Hill brings the zone of *Am. Humphriesianus* into close relation with the "Sands of the Upper Lias," and has caused it to be mistaken for the "Cephalopoda-bed" of Frocester and Leckhampton Hills. In the Northern Cotteswolds the Humphriesianus-zone is but feebly represented.

The Dundry Hill section was then described in a note by Mr. R. Etheridge, F.G.S., as comprising—1st (lowest), Lower Lias; 2nd, perhaps the "Lias Sands;" 3rd, the Shell-bed; 4th, Ammonite-bed (not equivalent to the "Cephalopoda-bed" of the Cotteswolds); 5th to 9th, shelly beds, ragstone, fine-grained oolite, and freestone; some of the latter representing the Parkinsoni-zone.

Dr. Wright then described the section in Gristhorpe Bay, from the Cornbrash to the Millepore-bed;—equal to the zone of *Am. Humphriesianus*. The fossils of these marine and freshwater beds were noted as existing in the cabinets of Leckenby, Bean, and others.

The zone of *Am. Parkinsoni* has the following synonyms, according to the author:—"Trigonia-grit and Gryphite-grit," Murchison and Strickland; "Ragstone and Clypeus-grit," Hull; "Spinosastage," Lycett; "Brauner Jura ϵ" (pars), Quenstedt; "Parkinsonthone, Brauner Jura δ und ϵ" (pars), Pfizenmayer; "Calcaire à Polypiers," Terquem; "Die Schichten des Ammonites Parkinsoni," Oppel. This zone is the most persistent of the three subdivisions

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of the Inferior Oolite, and is its only representative in the south-eastern parts of Gloucestershire.

The sections of Leckhampton Hill, Ravensgate Hill, Cold Comfort, Birdlip Hill, and Rodborough Hill afford the fossils and details illustrative of this zone.

In this communication Dr. Wright endeavoured to show that the Inferior Oolite of the South of England admits of a subdivision into three zones of life, and that each zone is characterized by the presence of Mollusca, Echinodermata, and Corals special to each. 2nd. That these three zones are very unequally developed in different regions both in England, France, and Germany, the individual beds composing the zones being sometimes thin and feebly developed (or altogether absent) in some localities, but thick and fully developed in others; the zone of *Am. Murchisonæ* is the one most frequently absent; that of *Am. Humphriesianus* has a wider area; and the zone of *Am. Parkinsoni* is the most persistent, is widely extended, and is very often the sole representative member of the Inferior Oolite formation. 3rd. That many *Lamellibranchiata* and a few *Gasteropoda* are common to the three zones, and that most of the *Ammonites, Brachiopoda, Echinodermata,* and Corals are limited in their range to one of the zones; but that each zone possesses a fauna which is sufficiently characteristic of it. 4th. The Parkinsoni-zone possesses many species of *Mollusca* and *Echinodermata* in common with the Cornbrash; and the Murchisonæ-zone, in like manner, contains many *Lamellibranchiata,* which appeared for the first time in the Jurensis-stage, although all the *Cephalopoda* of these two stages are specifically distinct from each other.

April 20, 1859.—Major-General Portlock, V.P., in the Chair.

The following communications were read :—

1. "On some Reptilian Remains from South Africa." By Prof. Owen, F.R.S., F.G.S.

Fam. Crocodilia. *Galesaurus planiceps,* the Flat-headed Galesaur (from γαλῆ, polecat, σαῦρος, lizard), a genus and species founded on an entire cranium and lower jaw. The skull in length less than twice the breadth, much depressed, and flat above. Occipital region sloping from above backward, divided by a high and sharp ridge from the temporal fossæ; these are wide and rhomboidal; orbits small; nostril single and terminal. Dentition, *i.* $\frac{4-4}{3-3}$, *c.* $\frac{1-1}{1-1}$, *m.* $\frac{11-11}{12-12}$; all the teeth close-set, except the intervals for the crowns of the long canines when the mouth is closed. Canines of the shape and proportions of those in *Mustela* and *Viverra,* without trace of preparation of successors in the sockets; of quite mammalian character. Incisors longish and slender, molars subcompressed; both with simple pointed crowns, of equal length, and undivided roots. Original transmitted to the British Museum by Governor Sir George Grey, K.C.B. From the sandstone rocks, Rhenosterberg.

Cynochampsa laniarius, the Dog-toothed Gavial (from κύων, dog, and χάμψαι, Egyptian name for Crocodiles, applied by Wagner to

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the Indian Gavial). This genus and species is founded on the rostral end of the upper and lower jaws of a Crocodilian Reptile, with a single terminal nostril, situated and shaped as in *Teleosaurus*, and indicating similarly long and slender jaws. Only the incisive and canine parts of the dentition are preserved; but these closely correspond with the same parts in *Galesaurus*, the incisors being equal and close-set, of simple conical form, and the canines suddenly contrasted by their large size. In shape they resemble closely the completely formed canines in Carnivorous Mammals. There is no trace of successional teeth. Original transmitted to the British Museum by Governor Sir George Grey, K.C.B., from Rhenosterberg, South Africa.

Fam. Dicynodontia. Subgenus *Ptychognathus*, Ow. (πτυχὸς, ridge, γνάθος, jaw).—This subgenus is founded on four more or less entire skulls, two retaining the lower jaw, referable to two species.

Ptychognathus declivis, Ow.—Plane of occiput meeting the upper (fronto-parietal) plane at an acute angle, rising from below upward and backward, as in the feline mammals; fronto-parietal plane bounded by an anterior ridge, extending from one superorbital process to the other; from this ridge the facial part of the skull slopes downward in a straight line, slightly diverging from the parallel of the occipital plane; superoccipital ridge much produced and notched in the middle; the occipital plane, owing to the outward expansion of the mastoid plates, is the broadest part of the skull, which quickly contracts forward to the ridged beginnings of the alveoli of the canine tusks; orbits oblong, reniform, suggestive of the reptile having the power of turning the eyeball, so as to look upward and backward as well as outward. Remains of sclerotic plates. Nostrils divided by a broad, flat, upward production of premaxillary, situated nearer the orbit than the muzzle, smaller than in type *Dicynodon*; temporal fossæ broader than long, and with the outer border longest; palate with single large oval vacuity, bounded by palato-pterygoid ridges; occipital hypapophyses proportionally thicker than in *Dicynodon tigriceps*; no trace of median suture in parietal, which is perforated by a 'foramen parietale;' frontals divided by a median suture and supporting a transverse pair of small tuberosities; anterior boundary-ridge of vertex formed by the nasals and prefrontals, the outer surface of both being divided into a horizontal and sloping facet; lacrymal bone extending from fore-part of orbit half an inch upon the face to the nostril; premaxillary long and single, its median facial tract flat, with a low median longitudinal ridge; maxillaries forming the lower boundary of the nostrils, and uniting above with the prefrontal, lacrymal, and nasal bones, their outer surface divided by the strong ridge suggesting the subgeneric name; teeth of the upper jaw restricted to the two canine tusks, the sockets of which descend much below the edentulous alveolar border; lower jaw edentulous, deep, and broad, with the fore-part of the symphysis produced and bent up to meet the seemingly truncate end of the premaxillary,—a

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character indicating, with the angular outline of the skull, the subgeneric distinction.

Ptychognathus verticalis.—The skull of this species, repeating the subgeneric characteristics of the foregoing, has the facial contour descending almost vertically from, and at almost a right angle with, the fronto-parietal plane. Orbits proportionally larger and more fully oval. Ridged sockets of the canine tusks descending more vertically from below the orbits. Originals transmitted to the British Museum by Governor Sir George Grey, K.C.B., from Rhenosterberg, South Africa.

Subgenus *Oudenodon*, Bain (*οὐδείς*, none, *ὀδούς*, tooth).—The skull in this subgenus presents the divided nostrils, the structure and the rounded contours of that of the true *Dicynodons*; also the same form, relative size, and position of the orbits and nostrils; but the zygomatic arches are more slender, straight, and long; and, although there be an indication of an alveolar process of the superior maxillary, the lower part of which projects slightly beyond the rest of the edentulous border of the jaw, it does not contain any trace of a tooth, so that both jaws are edentulous,—a character which had attracted the attention of their discoverer, Mr. Bain, who, in indicating it, proposed the name *Oudenodon*.

It is permissible to speculate on the possibility of these toothless Dicynodontoids being, after the analogy of the Narwhals, the females; or of their being individuals which had lost their tusks without power of replacing them, as the known structure of the true Dicynodons indicates. But there are characters of the zygomatic arches and temporal fossæ which differentiate the toothless skulls sufficiently to justify their provisional reference to a distinct subgenus.

Hyoid apparatus of *Oudenodon*.—Beneath one of the skulls, and imbedded in the matrix between the mandibular rami, were the following elements of the hyoid apparatus :—basi-hyal, cerato-hyals, thyro-hyals (or hypo-branchials), cerato-branchials, and uro-hyal.

The cerato-hyals are long, subcompressed, expanded at both ends; the thyro-hyals shorter and more slender; the cerato-branchials with a sigmoid flexure; the uro-hyal symmetrical, broad, flat, semicircular, with a production like a stem from the middle of the straight anterior margin. This apparatus shows the complexity by which the hyoid in Lizards and Chelonians differs from the hyoid in Crocodiles, and combines Chelonian with Lacertian characters. Transmitted by Mr. Bain from South Africa.

Dicynodon tigriceps.—Pelvis: ilium, ischium, and pubis coalesced to form an 'os innominatum,' with the suture at the symphysis obliterated. At least five sacral vertebræ; the first with broad, thick, triangular, terminally expanded pleurapophyses. The strong, straight, trihedral ilium overlies the above sacral rib, and extends forward to overlie also the last long and slender rib of the free trunk (thoracic) vertebræ. There are no lumbar vertebræ.

Pubis very thick, strong, with a broad anterior convexity resembling that of the *Monitor* in its internal perforation and external

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apophysis; ischium receiving the abutment of the last two pairs of sacral vertebræ.

The form of the anterior aperture of the pelvis is oval, with the sides broken by a slight angle at the middle, and the small end encroached upon by the slight angular prominence of the symphysis pubis. The long diameter is 11 inches (from the fore-end of the first sacral vertebra), the transverse diameter is 10 inches. The fore-half of this aperture is bounded by the first sacral vertebra exclusively, at the middle by its centrum, at the sides by its ribs; the hind-half of the aperture is bounded by the pubic bones. From the penultimate sacral vertebra to the symphysis pubis it measures 5 inches.

The outlet of the pelvis is of a semielliptic form, 9 inches in transverse, and 4 inches in the opposite diameter. Original transmitted by Mr. Bain from East Brink River, South Africa.

Crocodilia (?). Genus *Massospondylus*, Ow. (Gr. *μάσσων*, longer; *σπόνδυλος*, vertebra).—The author exhibited diagrams, and pointed out the characters on which he had founded (in the Catalogue of Fossil Remains of the Museum of the College of Surgeons) the genus *Massospondylus*, exemplified by the *M. carinatus*.

Genus *Pachyspondylus*, Ow. (Gr. *παχὺς*, thick; *σπόνδυλος*, vertebra).—The fossils exemplifying this genus form part of the same collection, obtained by Messrs. Orpen from sandstones of the Drakenberg range of hills, South Africa, and presented to the College of Surgeons.

2. "On the South-easterly Attenuation of the Lower Secondary Rocks of England, and the probable depth of the Coal-formation under Oxford and Northamptonshire." By Edward Hull, Esq., A.B., F.G.S.

By a series of comparative sections, made by actual admeasurements by the officers of the Geological Survey, it was shown that all the Lower Secondary formations attain their greatest development towards the north-west of England, and, on the other hand, they become attenuated, and in some cases actually die out, in the opposite direction. For example, it was shown that the Bunter Sandstone in Cheshire reaches a thickness of 2000 feet, in Staffordshire 600, and in East Warwickshire is absent; and a similar law of south-easterly attenuation was shown to maintain in the case of the Keuper, Lias, Inferior Oolite, and lower zone of the Great Oolite.

It was shown that the upper zone of the Great Oolite (the White and Grey Limestones of Wilts, Oxford, Lincoln, and Yorkshire) forms the first exception to the law; and from the fact of its occurrence in the Bas-Boulonnais below the Chalk, and resting on carboniferous rocks, the author inferred that it extends more or less uninterruptedly from England to France and Belgium, and southward to Mr. Godwin-Austen's palæozoic axis. The cause of this superior degree of persistency was referred to the organic, as distinct from the sedimentary nature of the formation, and its accumulation (like the White Chalk) on a deep sea-bed by the agency of Molluscs, Corals, and Foraminifera.

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It was shown that the Lower Permian beds are scarcely represented in Lancashire and North Cheshire, but that they attain their greatest development (1800 feet) along a band of country stretching west and east from Salop to Warwickshire, and the author traced the margin of the basin in which they were formed, along the west, north, and east. The *local* origin of these Permian beds, as having been derived from the Old Red and Silurian lands by which they were surrounded, was insisted upon, and especially as agreeing with the observations of Murchison, Ramsay, and other authors.

As contrasted with this local origin of the Lower Permian Rocks of Central England, it was shown that the sedimentary materials of which the Triassic Rocks are formed must have been drifted by an ancient oceanic current from a continent or large tract of land occupying the position of the North Atlantic, and that the sediment was spread over the plains of England as long as it was mechanically suspended. The increasing distance towards the south-east from the source of supply, accounted for the tailing-out of the sediment. During the Bunter Sandstone period, this sediment was drifted through the channel formed by the great headlands of Westmoreland and North Wales; but, as the whole area was gradually sinking (with occasional interruptions) during the periods of the Upper Trias and succeeding formations, the Welsh and Cumbrian mountains must have been nearly covered by sea at the close of the Liassic period.

The author adduced the following reasons for considering that the Bunter Sandstone of England formed dry land during the deposition of the Muschelkalk of Germany:—

1st. That the Lower Keuper Sandstone rests on an eroded surface of the Bunter; 2nd, that the basement-bed of the Keuper is frequently a breccia or shingle-beach; and 3rd, that there is a local unconformity observable in Stafford, Leicester, and Lancashire between these formations.

The author described the distribution of the quartzose conglomerates which form the middle division of the Bunter, and considers it probable that they are the reconstructed materials of the Old Red Conglomerate of Scotland.

The probable extension of coal-measures from the coal-fields of England to those of Belgium and France was considered, as also the bearing of the whole subject on Mr. Godwin-Austen's theory of the extension of coal-measures under the chalk of the Thames Valley; and it was inferred that coal-measures might possibly be found at not unapproachable depths under parts of Oxford and Northamptonshire. It was also shown that, from indications presented by the coal-formation at the southern borders of the Staffordshire and Warwickshire coal-field, there was reason to suspect that the formation becomes attenuated and less productive of valuable coal-beds in its extension towards the south-eastern districts.

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LXI. *Intelligence and Miscellaneous Articles.*

ON THE PHOSPHORESCENCE OF GASES BY THE ACTION OF ELECTRICITY. BY E. BECQUEREL.

IN the Memoirs presented by me to the Academy on the 16th of November, 1857, and 24th of May, 1858, relative to the luminous effects presented by bodies after having received the influence of light, I made use of tubes containing rarefied air, and in which were placed phosphorescent substances which became luminous after the passage of electrical discharges. Some time afterwards, M. Ruhmkorff, who arranged these apparatus in accordance with my directions, called my attention to the fact that in certain tubes containing only rarefied gases, which had been sent to him by M. Geissler, there were to be seen, after the passage of discharges, luminous traces persisting only for a few seconds, and analogous to those diffused by the phosphorescent substances employed in my investigations.

I have since studied the passage of electrical discharges through rarefied gases and vapours, which gives rise, as is well known, to effects of colour depending on their nature, with the view of ascertaining what are the gases which present the effect of persistence of light, and whether the phænomenon be analogous to the phænomenon of phosphorescence observed with solid bodies. In most tubes containing such gases as hydrogen, sulphuretted hydrogen, protoxide of nitrogen and chlorine, we observe faint gleams persisting after the passage of induction electricity, or even of a simple discharge of an electric battery, but the action appears to be limited to the internal surface of the glass tube. It is not due to phosphorescence of the glass; for tubes exposed to the action of a brilliant light, and then carried again into the dark, give rise to no action of this kind, and the phosphoroscope must be employed to observe the effects of persistence upon the glass, the duration of which is shorter than that which follows the action of electricity; the effect presented by tubes containing these gases would therefore appear to be the result of an electrization of the glass, or of the adherent gaseous stratum.

With oxygen a different effect is observed; when the discharges of a strongly excited induction apparatus are passed through a tube containing this gas in a rarefied state, and the passage of the electricity is suddenly stopped, the tube appears to be illuminated with a yellow tint, which persists for several seconds after the interruption, and decreases more or less rapidly according to conditions which I have not yet been able to ascertain. In order that the effect may be very manifest, the electricity transmitted into the gas must have a certain tension; it is therefore preferable to interpose a condenser in the circuit, and to excite sparks at a distance in the air between one of the conductors of the induction apparatus, and one of the platinum-wires penetrating into the tube. A simple discharge of an electrical battery of several jars produces the same effect. In order to observe the persistent luminous action, the operations must be carried on in the dark; care must also be taken to

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keep the eyes shut whilst the discharges are going on, and only to open them immediately afterwards, so that the retina may not be impressed at the moment of the passage of the electricity. The part of the tube in which the discharge takes place must be at least 15 to 20 centims. in length.

The peculiar action which illuminates the tube takes place between the actual molecules of the oxygen gas, and does not pass along the walls of the tube; for by making use of spheres of a capacity of 200 to 300 centims., the entire mass of the gas becomes opaline. By prolonging the tubes beyond the platinum-wires, it also appears that the rarefied oxygen beyond the part which directly receives the discharge, gives rise to an emission of light. On the other hand, this opalescence of the gas indicates that the effect does not result from electrical discharges due to the electrization of the glass, and which would traverse the space illuminated after the cessation of the inductive discharge, as it may be produced by friction of the outside of the tube.

When a tube is to give rise to an effect of persistent luminosity, there is produced, at the moment of the passage of the electricity, a yellow tint, which illuminates the mass of gas in the tube, and that independently of the different tints of the electric rays due to the intermixed gases; when this yellow tint disappears, the effect of persistence entirely ceases to be appreciable. It is even possible that gases mixed with oxygen may augment the duration of the persistence; for tubes, prepared apparently in similar conditions, furnished variable results as to intensity and duration.

If we operate with a small tube containing rarefied oxygen, after the electricity has passed for some time, the effect of persistence ceases to be appreciable; this result appears to show that the peculiar property in question disappears in the gas at the end of some time. Is it connected with the formation of ozone, which, in a determinate volume, cannot exceed a certain limit? This I have been unable to ascertain.

Sulphurous-acid gas sometimes presents an action analogous to that of oxygen; but the effect not being always exhibited, I have thought that it might depend on a partial decomposition of the gas and on a mixture of oxygen; the same is the case with rarefied air in the presence of phosphorus. However, I am at present following out these researches, and hope to ascertain, by means of an arrangement analogous to that which I have employed in the phosphoroscope, whether other gases and vapours besides oxygen would not give rise to effects of luminous persistence of shorter duration than that observed with the latter.

The phænomenon presented by oxygen, and perhaps in different degrees by other gases, probably depends on a peculiar action produced by electricity; for solar light, and even electric light itself does not give rise to any phosphorescence of this kind. Is it the result of vibrations impressed upon the molecules of the gases, or of a peculiar state of electrical molecular tension persisting for a few moments, or of some other physical or chemical cause?—*Comptes Rendus*, February 21, 1859, p. 404.

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THE

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[FOURTH SERIES.]

JUNE 1859.

LXII. *On the Periods and Colours of Luminous Meteors.* By J. H. GLADSTONE, *Ph.D., F.R.S.**

RECENTLY, on looking over the records of luminous meteors, several thoughts occurred to me which I believe are new; and some of them may be not unworthy of being brought under the notice of those interested in the subject.

PERIODICITY.—It is well known that showers of falling stars and meteors occur at certain annual periods, and that the two most remarkable of these are about August 10 and November 13. They have been observed on these two dates, not only in our own country, but on the continent of Europe, in America, and in India; and Humboldt has brought evidence to show that August 10 has had a reputation for meteoric displays for several centuries. It is not, however, every year that they are seen on the dates in question, even though the sky be clear; and they are sometimes reported as occurring a day or two earlier or later. This slight irregularity as to time may be one cause of their not being observed during some years, for it is only when the star-showers occur at night that they can be seen. A comparison of more ancient observations throws additional light on the nature of this periodicity. M. Chasles† has drawn up a catalogue of forty-six remarkable meteoric showers—or what are believed to have been such—between A.D. 538 and A.D. 1123; and here also we find recurrent periods, but different from those at present observed. Not one instance is noted of a shower occurring anywhere about the middle of either August or November. From the end of the sixth to the middle of the tenth century, but

* Communicated by the Author.

† *Comptes Rendus*, March 15, 1841.

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never since, showers occurred in February. The list commences with a shower on April 4, A.D. 538; but no similar date occurs for five centuries, till the four consecutive years 1093, 1094, 1095, and 1096, and again 1123.

The Chinese catalogues teach a similar lesson, and are peculiarly instructive when compared with that of M. Chasles. The records of meteors kept by that remarkable people extend, with some intermissions, from B.C. 687 to A.D. 1623, and have been given to the European world by M. Biot*. The French philosopher has drawn out a list of fifty remarkable showers; and, as in the list of M. Chasles, no date about August 10 or November 13 ever occurs in it. The 13th of November, however, is faintly indicated towards the latter part of the Chinese catalogues, but rather by the occurrence of single meteors than of showers. The 25th of that month and the end of August are both more frequently mentioned. The period of most common occurrence, and which ranges from A.D. 830 to 933, is about July 22; and during the Soung dynasty, from A.D. 960 to 1275, when the records were most carefully kept, meteors appear to have abounded two or three days later. And again, July 27 was marked by star-showers in Europe in A.D. 1784 and 1785. The period of March 23–29 occurs at intervals from B.C. 687 to A.D. 1062, both in the Chinese and European records; the Chinese also frequently notice showers of stars about October 14; and one of the showers of this date that occurred A.D. 934 is the only one that is mentioned in both lists, the only phænomenon of the kind that appears to have attracted attention at the same time in both hemispheres. There are indications of a periodical shower about ten days or a fortnight later in the year in the Chinese records of the sixteenth and seventeenth centuries; and Dr. Lardner† narrates similar appearances about the same time in the years 1202 and 1366.

These facts give little support to the ingenious hypothesis of M. Chasles, that there may be a secular progression of these periods, and that the showers of February, March, and April in the middle ages may be the same as now recur in August. It rather appears that the periods remain stationary sometimes for centuries, but that the transit of these streams of meteors through our atmosphere is liable to interruptions and changes from causes which we may speculate upon, but cannot as yet determine.

Colour.—Luminous meteors generally exhibit distinct colours; and these are recorded in the lists of observations. M. Poey of

* *Catalogues Générales des Etoiles Filantes et des autres météores observés en Chine. Mémoires des Savants étrangers*, vol x. pp. 129, 415.

† Museum of Science and Art, vol. i. p. 141.

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Havanna* has taken the trouble to go through the Chinese lists already referred to, the catalogue presented annually to the British Association by the Rev. Baden Powell, and the observations of M. Coulvier Gravier† at Paris, and from each of these sources to draw up tables showing the number of meteors in each month arranged according to their colours. The following Table is constructed from the totals of M. Poey's Tables, with some additional observations that he enumerates, but does not include in his lists; and with this difference also, that, for the sake of comparison, I have reduced the multitudinous shades mentioned in the observations to white, the six principal divisions of the solar spectrum, and red, yellow, green, and blue combined with white: thus, those given in the Chinese Tables as yellowish red, and those called reddish yellow, are classed together as orange; and again, bluish, whitish blue, and bluish white, are all counted as white-blue. The first three columns give the actual number of meteors observed, the last three the proportion per cent. of each colour.

Colours.	Chinese.	English.	French.	Chinese.	English.	French.
Red.....	51	129	4	5·1	12·2	6
White-red	5	52	6	0·5	4·9	9
Orange	567	112	4	56·8	10·5	6
Yellow	6	151	7	0·6	14·2	10·5
White-yellow ...	5	19	1	0·5	1·8	1·5
Green	0	6	0	0	0·6	0
White-green ...	0	6	1	0	0·6	1·5
Blue	8	326	0	0·8	30·8	0
White-blue.....	326	57	41	32·7	5·4	61
Purple.....	10	5	0	1	0·5	0
White	20	196	3	2	18·5	4·5
	998	1059	67	100	100	100

These three lists appear at first sight very different; and on the difference in colour exhibited in his Tables of the Chinese and of the English meteors M. Poey lays great stress. Yet this may be a difference rather in the expression than in the reality. There must always be some doubt whether the inhabitants of the celestial empire, one or two thousand years ago, meant by certain terms precisely the same colours as the French translator understood to be intended; and it is by no means unlikely that they grouped together as orange (or rather yellowish red, or reddish yellow) meteors which our countrymen, or our French neighbours, would class as red, reddish, or yellow‡. This is ren-

* *Comptes Rendus*, December 15 and 29, 1856; and January 12, 1857.

† *Ann. de Chim. et de Phys.* vol. xl.

‡ Since writing the above, I have been informed by Mr. Wm. Lockhart,

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dered the more probable when it is borne in mind that among terrestrial phænomena a red colour without any shade of yellow is rarely seen, and a yellow absolutely free from orange rays scarcely ever. A still clearer case of difference of expression occurs in respect to blue; there can be no reasonable doubt that we have generally called by that name meteors which, had they appeared in China or at Paris, would have been designated bluish or whitish blue. A possible reason for this will presently be given. Another point of apparent dissimilarity in the three lists, is in the much greater proportion of white meteors observed in England; but this also is susceptible of explanation. The fact is, that in the columns of the Rev. Baden Powell, "white" or "colourless" is always mentioned, while in the Chinese and French lists nothing is said about the colour of a meteor unless it has displayed some peculiar tint. Again, most of the observations which swell the number of white-blue meteors in the French list are described by M. Coulvier Gravier as "white becoming bluish near the horizon," and such would probably be designated merely "white" by the English observers. Were these excluded, the vast proportion of white-blue meteors in the French catalogue would be reduced to a per-centage not very dissimilar from that of the Chinese observations.

If, instead of the apparent differences, we remark the points of similarity in the three lists, we are at once struck with—

The small number of green meteors.—There is not a single Chinese observation of this colour. In the French and English observations, green occurs most frequently as a secondary colour which a large meteor exhibits in its course through the lower regions of the atmosphere. In Dr. Buist's records of Indian meteors*, green occurs more frequently in proportion: indeed he says, in writing to Prof. Powell, "The light of the larger meteors is generally orange, bluish, or greenish, hardly ever white†."

The small number of purple or violet meteors.—When this colour is remarked, it is also generally in meteors that change during their passage.

The absence of brown, except in one English observation which I have not included in the Table.

The fact that the large majority of luminous meteors exhibit a distinctive colour.—This remark applies apparently to the small shooting-stars as well as to the larger fire-balls.

for many years medical missionary at Shanghae, that the Chinese express orange by Hwang-tau, *i. e.* yellow-red. They have a specific word for green, and in general distinguish colours accurately.

* "Notices of the most remarkable meteors in India, of the fall of which accounts have been published," Bombay Geographical Transactions, 1850.

† British Association Report, 1849, p. 34.

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These may be nearly all divided into two great classes:—

1st. *Blue.*

2nd. *Orange, inclining more or less either to red or yellow.*—Under this head must be classed the red and yellow of the European observations, though even then the proportional amount will not equal that of the Chinese.

A large proportion of these meteors doubtless emit all the other rays of the spectrum at the same time, and in many cases they are emitted so equally that no preponderating tint is observed.

Change of colour of meteor.—Another fact of interest connected with the present subject is, the alteration that is frequently remarked in the colour of a meteor during its passage. This may be as follows:—

From white to reddish.
From white to bluish.

According to M. Coulvier Gravier, these two changes very frequently occur in meteors passing from the zenith towards the horizon. M. Poey endeavours to apply to the explanation of this fact the theory by which M. Doppler has sought to account for the changing colours of certain stars, especially among the binary systems. M. Doppler shows, on theoretical grounds, that "a luminous body moving towards the observer will change its colour from white in succession to the violet end of the spectrum, moving from the observer to the red." But he requires that the alteration of position of the luminous body and the observer should bear some near comparison in speed with the velocity of light—nearly 200,000 miles per second, whereas meteors fly at only about twenty miles per second on the average! A reason that appears more possible will be presently assigned.

From white to orange-yellow and blue-green.
From white to reddish and bluish, with reddish train.
From yellowish white to orange-yellow and to greenish white, being broken into several fragments, two of which passed from white to the colour of red-hot iron.
From orange-yellow to green.
From yellow and red to greenish yellow (fragments).

These also are given on the authority of M. Coulvier Gravier.

From white to red. A Chinese instance.
From blue to red.
From blue to green, and finally red.
From green to crimson.
From green to orange and red.

These are recorded in the British Association Catalogues, having been observed by Mr. Hind, Mr. Lowe, and others.

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Colours of train.—Many meteors in their flight leave a train behind them, which seems generally to be a faint luminosity, the colour of which observers have not usually noted. M. Coulvier Gravier finds the trains usually of the same colour as the meteor itself, but he mentions several cases of red trains left by meteors that became bluish as they approached the horizon. The converse of this has been observed both in China and England, namely a red star leaving a bluish vapour. Greenish trains are also not uncommon in the Paris catalogue. Red sparks proceeding from green meteors are recorded as seen both in England and in India, and the Chinese speak on one occasion of a score of little red stars jumping from a globe of fire. Where a meteor breaks up into smaller pieces, these not unfrequently present a different colour to the primary: thus an orange-red ball observed by Mr. Lowe, July 4, 1851, gave out many small separate balls which were bright blue, becoming purple; and the little balls shot from the meteor of November 13, 1803, are described as tinged with orange, yellow, and purple*. Yellow fragments have also been observed from a blue meteor; and the splendid vari-coloured fire-ball which flew across the north of England on April 27, 1851, left a train of yellow light.

The colour of the train sometimes changes; thus M. Coulvier Gravier relates instances of trains that became greenish after having been bluish or reddish, or both reddish and bluish, and another instance of a clear yellow train becoming deep red.

It not unfrequently happens that a meteor is followed a few seconds afterwards by a smaller one pursuing the same path. I and others have observed these to be of the same colour, but they do not appear to be invariably so.

Radiance different from apparent colour.—In descriptions of fire-balls, it not unfrequently happens that the narrator describes the luminous body as of one colour, while it casts a light of a different colour on surrounding objects. Instances might be quoted from the Chinese records, the most remarkable perhaps being that "a blue star spread a reddish glare which lighted the earth." Thus, too, in our own country a lady describes a brilliant meteor that, passing over Hampstead, "broke into an *intensely* radiant cloud," which threw on the walls of the houses a light brighter than that of the moon, but of a blue tint, though "there was no *blue* light in the cloud itself." The explanation of this apparent paradox will be given below.

Sources of error.—In discussing these reputed facts, certain illusions to which observers are subject must be taken into account. Thus there is at the very outset the difficulty that *different observers often call the same colour by different names.* This

* Phil. Mag. First Series, vol. xvii. p. 279.

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has been already adverted to; and indeed it is a matter of daily observation, that two parties, both perfect in their perception of colour, will differ in their mode of explaining the tint even of stationary objects; how much more may we expect them to differ when a coloured light is suddenly presented up in the sky, and as suddenly disappears! Cases in point are not wanting. A meteor which Mr. Lowe designated as yellow, appeared "of a beautiful clear blue colour" to Mr. T. W. Webb*. But a more curious instance occurred last autumn. The meteor of September 12, 1858, was described in the *Times* newspaper by several eye-witnesses, of whom F. A. B. states it to have been "green at first;" N. R. "green surrounded by white;" W. Rowlett, "white;" and T. W. "vivid whitish blue;" while B. H. declares it was "primrose." Such opposite statements would overthrow entirely our confidence in the recorded observations, were it not that the greatness of the discrepancy leads us to the belief that the meteor in question must have been one that changed in colour, and thus actually presented a different appearance in this respect to observers in different places.

It is also quite possible that *a meteor may emit rays which in the aggregate would produce one colour, and yet may affect the observer with a sensation of a different colour.* This may arise from absorption, intensity, or contrast.

Absorption.—Thin mists and long reaches of air have a tendency to absorb or disperse the more refrangible rays emitted from a luminous body, while they suffer the less refrangible to pass. Thus everyone has noticed how red the sun, and how orange the moon appear under certain circumstances, especially when near the horizon. This effect must frequently be produced on the light of meteors during their passage through our atmosphere.

A row of street lamps in misty weather exhibits the same phænomenon on a smaller scale; and it may be observed even in clear weather by looking, not at the jets of flame themselves, but at the streamers which appear to issue from them when we nearly close our eyelids. The streamers from the nearer gas-lamps appear yellowish white, from those further removed greenish or orange, and from the most distant almost red. A candle seen through a mixture of a little milk with a large quantity of water, also exhibits in a striking manner the non-transmission of the more refrangible rays.

This suggests a simple explanation of the fact, that those meteors which appear to change colour during their passage through the misty skies of England almost invariably terminate in red. It may not, however, be the sole reason.

* British Association Report, 1852, p. 189.

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Intensity.—Helmholtz has shown that light of almost any degree of refrangibility, if very intense, gives a sensation of whiteness; and perhaps of all rays, the blue exhibits this in the most striking manner.

This will account most satisfactorily for two separate facts already adverted to. The one is the apparent paradox of a white meteor shedding a blue radiance. The intensely brilliant cloud over Hampstead was doubtless emitting blue rays in the greatest proportion, but on account of its brightness the blue tinge could not be observed except as reflected from distant objects. The other is the phænomenon so frequently observed by M. Coulvier Gravier, of a white meteor becoming bluish or reddish as it approached the horizon. Indeed it might have been predicted that many a slightly-coloured meteor would appear white when in the zenith, and thus nearest to the spectator; and that as it passed to a distance, its luminosity being diminished, its proper colour would become evident. This may also be one reason why so many meteors are called blue by English observers, while under the clearer skies of the Continent and China they are designated white-blue.

All who have been in the habit of observing the solar spectrum, must have remarked how the apparent colour of the space between the fixed lines D and E varies with the intensity of the light; what appears green when diffused light is examined, is yellow when the rays come direct from the sun itself. Proofs might easily be multiplied of the tendency of yellow light, when rendered dull, to give the impression of green.

This suggests a simple physical cause for what is frequently noticed in the Paris observations,—a yellowish meteor becoming more or less green as it passes away from the spectator.

Contrast.—The apparent colour of an object is always affected to a greater or less degree by the colours of surrounding objects. This source of error must also come into play with meteors; and perhaps one of the results will be, that the sparks or fragments thrown off from the luminous globe may appear more distinct in colour than they really are.

Period and Colour.—If the cosmical theory of the origin of meteors be the correct one, there would be no improbability in supposing that the stream of little bodies revolving round the sun, and crossing our orbit at one period of the year, should differ in composition from those that cut our path at another period. If different in composition, they would probably exhibit different colours in combustion. It became an interesting point, therefore, to examine whether the catalogues show any indication of this.

On examining the Chinese records, we find that the predomi-

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nating colour of a great shower of falling stars is very rarely given; and on consulting M. Poey's monthly tables, little can be observed beyond the fact that the blue meteors are more numerous in comparison with the orange during the months of August, September, October, and November, than during the rest of the year. M. Poey also makes the singular observation, that the Chinese meteors "show a remarkable constancy of tints during a long period of years; when an equally constant but different scale of colour prevails, and this for several successive periods."

On turning to the monthly tables of the English observations, we at once remark a great difference in the relative proportions of the different colours. Thus, to confine our attention simply to the months of August and November, when the great showers usually occur, we find a difference that cannot be attributed to mere accident. In the following Table, the first two columns give the actual number of observations in each month according to the colour; the second two, the same numbers reckoned to a hundred parts; and the fifth column, the average proportion of the colours for the whole year as given in the Table on a preceding page. There happens to be no observation of green or purple during either of these months; and for the sake of condensation I have added together the red and white-red, yellow and white-yellow, blue and white-blue.

	August.	November.	August.	November.	Whole year.
Red.....	49	24	15·6	21·6	17·1
Orange	8	23	2·6	20·8	10·5
Yellow	44	16	14·0	14·4	16·0
Blue	164	30	52·2	27·0	36·2
White.....	49	18	15·6	16·2	18·5
	314	111	100·0	100·0	98·3

Here we see at once that about an average amount of yellow and of white meteors (or a little less) fall in each of these months; but August is marked by a great deficiency of orange, and a great excess of blue meteors, while, on the contrary, November exhibits comparatively few blue, and a very large proportion of orange meteors, with a slight increase also of the red. On looking at the grand displays about August 10 in the lists of the Rev. Baden Powell, we note the large proportion of blue meteors which stream across the heavens at that period, a phænomenon I myself had the fortune to witness last year. The November showers are not so distinctly marked in Prof. Powell's catalogue; but there is enough to indicate how meteors emitting the less refrangible rays abound at that season.

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If all these luminous meteors are produced, as some of them certainly have been, by the combustion of those solid masses of metal and stone which occasionally strike upon the earth, we might possibly learn the composition of a particular meteor by the colour it displays. But this is a very complicated problem. We know of what substances meteorites usually consist; but each one contains several elements which may all glow together, and may burn either together or separately. Iron is a constituent seldom wanting; and this metal, when heated, exhibits first what is called "dull redness," but which consists principally of orange and even green rays; as the heat increases, it becomes "bright red," and then emits true red rays in large quantity: when it catches fire in the air it scintillates, giving forth blue rays in addition to those already mentioned, and appearing of a reddish-white colour. It can scarcely be doubted that many of the red and white-red meteors, especially those that throw off red sparks, consist of this metal. If iron, whether meteoric or otherwise, be ignited in the broken galvanic circuit, the electric light is superadded to that of the burning metal, producing an intensely luminous flame which appears white, but casts a bluish radiance on surrounding objects. Nickel when ignited displays a somewhat larger amount of green rays. Meteoric iron always contains more or less sulphur and phosphorus: the last of these elements in burning emits all the rays of the solar spectrum: the blue colour of burning sulphur is well known; yet it differs greatly according to the intensity of the combustion. If iron pyrites be set on fire in air, the flames of both elements become distinctly visible, and the sulphur will continue to burn after the iron has ceased to do so. The cobalt, zinc, lead, and other metals occasionally found in meteorites will of course burn with their distinct flames; and the silicate of magnesia or other minerals are capable of becoming brilliantly incandescent.

LXIII. *On the Law of Electrolytic Conduction.* By M. Buff. *In a Letter to* Mr. Faraday*.

My dear Sir,

A FEW years ago you raised some doubts as to the generality of the law, that in compound fluids there is no conduction without decomposition. These doubts referred to our imperfect acquaintance with the deportment of the more complicated compounds, and to the property possessed by several electrolytes to conduct electricity when in the solid condition.

* Communicated by Professor Faraday.

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Last winter I was able, through friendly assistance, to examine the electric deportment of a number of compound bodies. In your 'Experimental Researches' you had stated that higher compounds, such as sulphuric acid, when they enter into further combinations, do not suffer a primary decomposition by the electric current. I have satisfied myself in the first place that the few exceptions to this rule which it was believed had been discovered, do not exist. My mode of operation consisted in decomposing the fluid, in a V-shaped tube, under circumstances which permitted me to recognize the direction in which the ions move towards the electrodes. In this way it was found that chloric, iodic, chromic, and manganesic acids and salts are decomposed exactly in the manner which Daniell proved regarding nitric and sulphuric acid.

Of pure specimens of the higher degrees of combination we have unfortunately but a limited choice. Many of them are too refractory, or suffer decomposition at high temperatures; others, which are easily obtained in a liquid state, are not conductors. Nevertheless some sufficiently fused Cu^2Cl was found to conduct well, and according to the results of several analyses, split itself into Cu^2 and Cl. In like manner, basic protonitrate of mercury contains for 2Hg one O. The alcoholic solution of sublimate, when free of hydrochloric acid, conducts very badly, but is decomposed into Hg^2Cl and Cl; in which case, however, I will not venture to decide whether or not the Hg^2Cl be formed by secondary combination, or by the action of Hg upon the surrounding HgCl. In all these cases, whether a simple or compound atom was liberated, the amount of decomposition agreed very well with the indications of the voltameter. As voltametric medium, I soon chose the precipitate of silver from nitrate of silver. The latter serves best when only feeble currents are to be obtained.

Anhydrous chloride of aluminium, easily fusible, and a good conductor, was decomposed into chlorine and pure aluminium. Molybdenic acid and vanadic acid were decomposed, so that for one atom of MO^2 or VO^2 we had one of O. Not a trace of metal was obtained at the same time.

Acid chromate of potash is known to contain the second atom of acid free from water. Fused, it conducts very well. In this case, according to several analyses conducted with care, the current divides itself between the free acid and the salt. The first was decomposed into Cr^2 O^3 and O^3, and the last into K and CrO^4. The sum of both decompositions agrees in the most exact manner with the electrolytic law, when we assume that in the decompositions and compositions proceeding in different directions, an atom of Cr^2O^3 carries the same quantity of electricity as three atoms

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of K. When Cr^2O^3 was still further electrolysed, as is the case with Al^2Cl^3, then in this new decomposition Cr^2, or two atoms of chromium, would have the same signification as three atoms of K. Unfortunately I could not examine oxide of chromium in a fused condition. I believe, however, that the deportment of chloride of aluminium is sufficiently convincing. The electrolysis of higher degrees of combination is as equally reconcileable with the fundamental law of electrolysis as in the case of chemical combination the multiple proportions are with the law of equivalent .

Several of the bodies examined, as protochloride of copper, chromate of potash, or chloride of lead, I found to be sufficiently good conductors at ordinary temperatures, to be able to measure this capacity by a precipitate of silver, which undoubtedly was very small. By heating without fusion, the conductivity quickly augmented, and certainly without any real decomposition taking place. When a platinum wire surrounded by several layers of solid and fused chloride of lead was dipped into a liquid chloride of lead, which was not much or not at all heated above the point of fusion, a new layer of the chloride naturally deposited itself round the wire. If, however, by the immersion of the wire, as the pole of a battery, a strong electric circuit was at the same time closed, the temperature of the substance was so raised by the current passing through it, that it was fused off in a few seconds. If the chloride of lead could retain such a conductive power in the liquid state also, in its electrolysis a great precipitate must show itself. This, however, did not occur. Small differences, which certainly occur, and which were also observed in your electrolyses, stand in no relation whatever, and may be referred to other causes,—for example, to the no small conductivity of the hot sides of the glass vessel. With chromate of potash and protochloride of copper, I found the same as with chloride of lead. The copper salt in its solid state showed itself so sensitive to changes of temperature, that even by the heat of the hand the conductivity was considerably increased. Still the electrolysis gave a satisfactory result. From these experiments we may certainly conclude as to the deportment of other electrolytes; and it is perhaps not too bold to assume that even oxide of iron and sulphide of lead, if they could be obtained in a state of fusion and subjected to a current, would conduct only so far as they are decomposed.

I remain, dear Sir,

With high esteem,

Your obedient Servant,

H. Buff.

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LXIV. *On the Thickness of the Earth's Crust. By the* Rev. Samuel Haughton, *F.R.S., Professor of Geology in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your May Number, the Archdeacon of Calcutta has published an interesting Paper "On the Thickness of the Crust of the Earth," on which I would wish to offer a few observations.

In noticing my own investigations on this subject, he has accused me of a fallacy in reasoning, and adds that consequently "my conclusions do not prove anything whatever regarding the proportion of the solid to the fluid parts."

My object, in the paper commented on, was to show that no investigations on this subject were of any value, inasmuch as they all rested on *arbitrary* hypotheses, among which I include the law of density $\left(\frac{\sin qa}{a}\right)$, and all guesses as to the ratio of the specific weights of the solid crust and liquid nucleus, and friction between them. At the close of my paper, I have given an example of the mode of calculation to be employed, if we had the requisite knowledge of the specific gravities and law of density of the crust and nucleus. As my object was merely to give an example, I have chosen a sufficiently absurd hypothesis: viz. specific gravity of outer shell 2·75, and of whole earth 5·50, and the whole composed of two homogeneous parts; required to find the thickness of the shell. The answer to this problem comes out to be 768 miles, which Archdeacon Pratt, by some strange misconception of my meaning, takes to be my deliberate conclusion as to the thickness of the crust of the earth.

The fallacy in reasoning of which I am accused, is deliberate, and consists in assuming the same law of ellipticity and density to hold both for the crust and nucleus.

I do not know whether it does or not; but no one else knows anything *positive* about it, and I am therefore entitled to deny the validity of any determination of thickness of the earth's crust, based upon any arbitrary assumption of a law of density, of which I know nothing. It has always appeared to me that the speculations of mathematicians respecting the interior of the globe were as unfounded and unreal as their speculations respecting the supposed luminiferous medium. We are equally ignorant in both cases; and our calculations must be only regarded as so much useless and learned labour in vain.

I believed, when I wrote my paper on the original and actual fluidity of the earth and planets, and still believe, that the thickness of the crust of the earth, for anything we know

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to the contrary, might have any value from 10 miles to 4000 miles; and I regret extremely that I should have expressed myself so carelessly, as to leave the impression on the mind of so good a mathematician as Archdeacon Pratt, that I had seriously attempted to calculate the thickness of the earth's crust, if it has a crust at all: I would rather that he should class me with those mathematicians who periodically square the circle and discover perpetual motion.

To show how little is known of the simplest elements of this problem, which Archdeacon Pratt thinks Mr. Hopkins has solved, I may quote the following passage, with which he introduces his own theorem respecting the crust, derived from the consideration of the Himalaya Mountains:—

"It has been suggested that the crust may project downwards into the lava so as to be supported by buoyancy. But this will not produce the desired effect; for the crust being formed from the liquid, will have pretty nearly the same density as the parts of the lava from which it was formed; *if anything, it will be somewhat more dense.*"

I have never before met a student of nature who held this opinion—which is highly improbable when we consider that all metals float in their respective liquids at the melting-point, and that lava will float on liquid lava.

How can the Archdeacon suppose there is any value in Mr. Hopkins's determination of the thickness of the earth's crust—based as it is on the *impossible* absence of friction, and on Laplace's *arbitrary* law of density—when we find him at the same time basing a theory of his own on a physical assumption contrary to all our experience?

In my humble judgment, if there be a liquid molten mass beneath our feet, the Himalaya Mountains float on its surface, as an iron bar would float on a bath of mercury, an iceberg on the surface of the sea, or a penny bun in a basin of milk.

I am, yours faithfully,

Trinity College, Dublin,
May 16, 1859.

SAMUEL HAUGHTON.

LXV. *Description of an Apparatus for examining the Electrical relations of unequally heated Mercury and Fluid Alloys in conducting Liquids.* *By* G. GORE, *Esq.**

[With a Plate.]

IN a former Number of the Philosophical Magazine (January 1857), I described an apparatus for examining the electrical relations of unequally-heated metals in liquids; but that appa-

* Communicated by the Author.

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ratus was limited in its use to such metals and alloys as are *solid* at ordinary temperatures; the present one is designed for similarly examining such metals and alloys as are *fluid* at 60° F.

Similar letters in the three figures indicate similar parts of the apparatus.

Plate II. fig. 1. A is a square wooden base, 10 inches long and 5 inches wide; B and B′ are two vertical (mandril drawn) tubes of brass, 12 and 10 inches long respectively, fixed firmly into the wooden base; C, C′, and C″ are horizontal pieces of brass tubing, $1\frac{1}{2}$ inch long × 1 inch diameter, $1\frac{1}{4}$ inch long × $\frac{7}{8}$ inch diameter, and $1\frac{1}{8}$ inch long × $\frac{5}{4}$ inch diameter respectively, each perforated across its axis with two holes of such a size as to enable them to slide up and down upon the vertical tubes with a moderate degree of freedom; they constitute the supports for the apparatus and spirit-lamp, and enable these to be shifted in position either vertically or horizontally round the uprights. D is a cork (or a piece of brass tubing split at one end) about $1\frac{1}{2}$ inch long, fitted rather loosely into the tube C; it forms the chief support for the apparatus by means of the metallic cross-tree D′, and gives additional control over the position or level of the apparatus by being easily turned upon its axis in the tube C. The cross-tree consists of two horizontal thin strips of elastic (hammered) brass or German silver, about $5\frac{1}{2}$ inches long and $\frac{1}{2}$ inch wide at its middle part, tapering in width to $\frac{3}{8}$ inch at the ends; they are soldered together at the middle part, and fixed securely to the cork with their edges uppermost, either by simply making a vertical saw-cut in the upper part of the cork and forcing the strips into it, or more securely by first enclosing the outer end of the cork in a closely-fitting short brass tube, making a similar cut in it and the cork, and forcing the strips into it; the strips are bent to the form represented in fig. 2, and are held together closely at their ends, when in use, by means of small metal clips E, E, figs. 1 and 2. The apparatus may, by means of the cross-tree, be bodily removed from connexion with the stand, and without much risk of accident discharged of its contents.

The chief part of the apparatus consists of two tubulated glass bulbs, F and G, of equal size, varying from $1\frac{1}{2}$ inch to 2 inches in diameter; they are supported by means of the strips of metal, D′, clasping their upper tubulures at H; F is provided with a glass tube, I, open at both ends, 1 inch long and $\frac{5}{16}$ inch in diameter, proceeding *upwards* in an inclined direction towards the bulb G; it is also provided with another open glass tube J. G has a similar tube, K, inclined *downwards*, of the same size as I, and connected with the latter, water-tight, by a short piece of vulcanized india-rubber tubing L; it has also another open glass tube, M, about 8 or 10 inches long (or more), and $\frac{1}{8}$th or

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$\frac{1}{10}$th of an inch in diameter; M is supported at its outer end by means of a cork, N, and a piece of bent sheet brass, O (see fig. 3), fixed in the tube C″. P is a small moveable wooden screen, 3 inches wide and 3½ inches long, supported by means of a wire (with a handle, Q), which passes first in a vertical direction through the cork D, and then horizontally outwards beneath two small wire hooks fixed in the upper and back side of the screen. R and R′ are two small solid stem-thermometers with cylindrical bulbs, supported in the necks of the glass vessels by means of closely-fitted and perforated corks; the cork of the flask G has a small notch cut in its side to allow expanded air to escape. S is simply a small spirit-lamp capable of being moved with its stand T, either vertically or horizontally, by means of the tube C′.

Mode of using the Apparatus.

Take about half an ounce by measure of clean mercury, or of the fluid alloy under examination, pour a portion of it into the outer end of the tube M until it acquires the level U in the bulb G, and pour another portion into the outer end of the tube J until it attains a similar level.

The liquid to be examined having been filtered and recently boiled to expel dissolved air, and cooled to the atmospheric temperature, is poured into the large tubulure of F (the thermometer having been previously removed) until the level V V in the two vessels is attained; the thermometer is then replaced air-tight. The extreme ends of mercury in the tubes J and M are now connected by wires, W W, with a galvanometer, and the apparatus allowed to remain at rest until the needles return to zero. Heat is now applied to G *very gradually* by means of a very small flame of a spirit-lamp, the latter being in its lowest position, and is gradually increased by slowly raising the lamp-stand, —the deflexions, their amount and direction, and the temperatures of the contents of the bulbs being recorded at intervals until the liquid in G approaches its boiling-point. The heat is effectually excluded from passing to the vessel F by means of the inclined position of the connecting tubes I and K, also by means of the screen P.

If the liquid is not pre-boiled, interferences are produced by the development of air-bubbles upon the surface of the metal in G at the higher temperatures. If the heat is applied too suddenly, the apparatus is endangered; and if applied too rapidly at the higher temperatures, large bubbles of vapour are formed at the lower surface of the mercury. If by long-continued action heat is suspected to pass by conduction to the outer end of the mercury in M, a small piece of damp cloth may be placed about mid-distance upon that tube. If the corks surrounding

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the thermometers are well fitted, and the notch in the side of the cork of R′ closed by a fragment of bees'-wax, the apparatus may be safely left for many hours without risk of the liquid absorbing atmospheric air.

In order to diminish the weight of the mercury without decreasing the amount of its acting surface, I have in some instances formed the bulbs with flat bottoms, and in other cases have made them of the shape of a turnip, thereby diminishing the weight of the liquid also.

In attaching the india-rubber tube L, the bulbs should be held by the tubes I and K only, because the apparatus is very fragile.

Birmingham, May 9, 1859.

LXVI. *Theoretical Considerations respecting the relation of Pressure to Density.* By Professor Challis*.

A THEORY of physical forces, such as that which I have indicated in preceding communications, while it may be proved to be false by a single contradictory fact, in the absence of such proof receives confirmation by every additional fact, or class of facts, which it explains. I have to a considerable extent shown the consistency of the hypotheses of the theory with phænomena of light; and in the Philosophical Magazine for last March I gave the principles of an undulatory theory of heat based on the same hypotheses. To this communication I propose now to revert, for the purpose of inquiring how far the views there advocated are consistent with what is known by experiment respecting the relations between *pressure* and *density*.

The fundamental hypotheses above referred to are these only:—The ultimate atoms of cognizable bodies are inert, hard, and spherical, and act upon each other by the intervention of the undulations of a continuous and highly elastic medium pervading space. In the article just cited, it was found on hydrodynamical principles that each atom is an origin of reflected undulations, and that the velocity V and condensation σ, at any distance r from its centre on the prolongation of a given radius, and at any time t, are given generally to the first approximation by the equations

$$V=\frac{f'(r-\kappa at+c)}{r}-\frac{f(r-\kappa at+c)}{r^2},$$

$$\kappa a\sigma=\frac{f'(r-\kappa at+c)}{r}.$$

Further, it was shown that the central velocity expressed by the

* Communicated by the Author.

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second term of the value of V, gives rise to a *repulsive* action of each atom on neighbouring atoms varying inversely as the *fourth* power of the distance, and that the velocity expressed by the first term, and accompanied by the condensation σ, also causes a repulsion between neighbouring atoms, but of incomparably less magnitude than the repulsion of the first kind. Since, however, this part of the velocity, as well as the condensation, varies simply as the inverse of the distance, it appeared that the dynamic effect of an *aggregation* of atoms on any single atom might be of sensible magnitude, and be either repulsive or attractive according to the distribution of the condensation of the incident waves about the surface of the atom.

Now quite apart from the above theory of molecular action, a general expression for the resultant of the action of surrounding atoms on a given atom may be obtained as follows, on the hypotheses that each atom is insulated, and that the forces urging it are sensible only at insensible distances. Suppose the collective atoms to constitute a medium, either solid, liquid, or aëriform, and conceive a surface of equal pressure and density of the medium to pass through the position A of a given atom. Then the resultant of the molecular action on the atom is plainly in the direction of a normal to this surface, and tends from the denser to the rarer part of the medium, or from the rarer to the denser, according as the component action is repulsive or attractive. Within the sphere of molecular activity the change of density $\Delta\rho$ corresponding to a given change Δz of the distance from the tangent-plane at A may be considered constant: or D being the density at A, the density ρ at any distance z from the tangent plane is $D+Qz$, Q being constant. Hence if r be drawn from the atom in any direction making an angle θ with the normal, and $\psi(r)$ express the law of the aggregate action of the atoms included in the small space

$$\Delta r \times r\Delta\theta \times r\sin\theta\Delta\eta,$$

the number of which is proportional to ρ and is supposed to be very large, then the resulting action in the direction of the normal is proportional to

$$\Sigma . \Delta r . r\Delta\theta . r\sin\theta\Delta\eta(D+Qr\cos\theta)\cos\theta\psi(r).$$

If this quantity be treated as an integral, and the integration be taken from $\eta=0$ to $\eta=2\pi$, and from $\theta=0$ to $\theta=\pi$, the result is

$$\Sigma . \frac{4\pi r^3 Q\psi(r)}{3}\Delta r.$$

Or, integrating from $r=0$ to $r=$ infinity, because by hypothesis the values of $\psi(r)$ are insensible for all but very small values of r,

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the amount of accelerative force irrespective of its sign is $H.Q$, or $H.\frac{\Delta\rho}{\Delta z}$, H being a certain constant.

Let us now suppose that each atom of the medium is also acted upon by an extraneous force, as the earth's gravitation, and that this force just counteracts the resulting molecular force. If the extraneous force be G, since there is equilibrium of the insulated atom, we must have

$$G = H.\frac{\Delta\rho}{\Delta z}.$$

But the pressure being p, and being due to the action of the force G, we shall have by hydrodynamics,

$$\Delta p = G\rho\Delta z.$$

Hence

$$\Delta p = H\rho\Delta\rho;$$

and by integration,

$$p = \frac{H}{2}\rho^2 + C.$$

We have thus been conducted to a law connecting the pressure and the density, different from that of Mariotte, by a process which assumes only that media are molecularly constituted, and that molecular action is insensible except at insensible distances. To account for this difference is a matter of some scientific interest. Since experiment has shown that for the earth's atmosphere, the temperature being given, $dp = G\rho dz = K dp$, K being constant, it follows that $\frac{d\rho}{dz}$ varies as ρ; that is, the difference of the mean densities of two contiguous atmospheric strata of given small thickness is greater as the density is greater. And yet the resulting molecular action of the strata on a given insulated atom is everywhere equal to the force of gravity. Not having met with any explanation of this fact, I proceed to apply to it the theory of molecular action I have proposed above.

According to this theory, besides the repulsive action of individual neighbouring atoms on a given atom, varying inversely as the fourth power of the distance, there may be a repulsion due to the condensation of waves propagated from an aggregation of atoms, varying *cæteris paribus* according to the distribution of the incident condensation about the surface of the given atom. This repulsion will be less as the density of the medium, and consequently the condensation of the aggregate waves, is greater, because, as the condensation increases, the pressure on the hemisphere of the atom on which the waves are incident dif-

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fers less from that on the opposite hemisphere. Hence the constant H, which in the above analysis was supposed to be independent of ρ, must, in order to take into account the effect of this distribution of pressure, be regarded as an unknown function of ρ. The law of Mariotte requires that this function should be of the form $\frac{H}{\rho}$, and that the repulsive action from the individual atoms should not be of sensible magnitude. The small amount of this repulsion in aëriform bodies may well be attributed to the comparatively large mutual distances of the atoms. It is clear that a repulsion varying inversely as the fourth power of the distance, if sensible between neighbouring atoms in a gaseous substance, would be enormously great if the same substance were reduced to a liquid or solid state.

This law of the increase of the repulsion of aggregation with diminution of density, seems to account for a phænomenon otherwise very difficult to explain, viz. the great development of repulsive action in the attenuated tails of comets, the effects of which were so signally exhibited in the recent instance of Donati's comet.

In liquid and solid bodies, on the contrary, by reason of the much greater contiguity of the atoms, the repulsive action from the individual atoms becomes very energetic, while for the same reason the condensations due to an aggregation of atoms may be so far increased, that the pressure about any atom on which they impinge may either be distributed uniformly, or may accumulate on the side opposite to that of incidence, and thus the repulsion be converted into attraction. It is also conceivable that under these circumstances the molecular action, whether repulsive or attractive, may be nearly proportional to the number of atoms in a given space, and that for, at least, small variations of the density of the medium, the quantity H is very nearly constant. The law connecting the pressure and density in liquid and solid bodies would, under this limitation, be expressed by the formula $p = \frac{H}{2} \cdot \rho^2 + C$.

This theory enables us to conceive how a gaseous body, by being greatly compressed, may be converted into a liquid.

Cambridge Observatory,
May 18, 1859.

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LXVII. *Some occasional Observations on the Structure, the Melting, and the Crystallization of Ice, made in Siberia.* By A. Erman, *Prof. Univ. Berol.*

To John Tyndall, Esq., F.R.S.

Dear Sir,

YOUR important experiments and observations on the physical properties of ice, of which I have read the report in the Philosophical Magazine, No. 108, remind me of some remarks on the same subject which occurred to me during my travels in Siberia. It seems that at present these remarks may become useful; for some of them agree perfectly with the results which you have already obtained, and therefore confirm them, and some others, like all phænomena that are still waiting for an explanation, may be an inducement to new experiments. I think, therefore, that either yourself in the second part of your researches, or other observers who may participate in them, would like to take notice of my observations. If you are of this opinion, I should wish the description of what I have given to become known to English philosophers, by your publishing it in the Philosophical Magazine. I have endeavoured to have these observations quite verbally translated from the German volumes, entitled *Reise um die Erde, u. s. w.*, which I published in the years 1835 to 1849; and I hope you will convince yourself of this agreement by comparing them with the passages quoted from the German work. I may add that my translation of many of the passages in question agrees fully with that of Mr. W. D. Cooley, in his somewhat abridged English edition of part of my book, entitled "Travels in Siberia," &c., by A. Erman: London, 1848. The remainder of the annexed remarks, however, will be found in full accordance with the German only, whereas some particulars have been omitted in the English abridgement, perhaps as not being of interest to the public in general. I have nevertheless quoted at each of these passages the pages of the English as well as of the German edition, when the phænomena in question are either merely alluded to, or fully described.

Believe me, dear Sir, to be with sincerest regard,

Your obedient Servant,

Berlin, 122 Friedrichstrasse,
April 23, 1859.

A. Erman.

Structural divisions of ice observed on Lake Baikal.

(*Reise um die Erde, u. s. w., Histor. Abtheilung*, vol. ii. p. 175. Travels in Siberia, &c. vol. ii. p. 284.)

February 26. . . . . We reached the coasts of the Baikal at Posolskoi about three o'clock in the afternoon, and commenced

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immediately the passage over it to Cadilosaja. The crumbling of the ice into upright flakes extends for about a verst from the bank; and then begins the level glossy surface. We made a magnetic observation on the lake, when we had reached a distance of about ten versts from the shore at Posolskoi. The ice afforded our instruments a perfectly firm foundation, and was at the same time more free than any rock can be, from the suspicion of magnetic attraction. It is perfectly transparent, but traversed by perpendicular cracks, by means of which we were enabled to discern where the fluid and dark-green water began, and to estimate the thickness of the ice at 4 feet. These cracks were all extremely narrow, and filled only with air. Many of them reached from the surface only to a certain depth, which was the same for all, and seemed to be a third of the entire thickness of the ice. The other cracks then began at this depth and reached down to the water. I remarked, moreover, that the planes which these cracks affect, intersect one another at an angle of 120°; so that both the upper and lower strata are thereby divided into prisms, which have nearly the same kind of regularity as basaltic columns, but with a much greater breadth. It was evident that this separation must have taken place during the congelation of the ice, and that this again took place at two different times, and instantaneously in each of the two strata alluded to. It may be conceived that the uppermost layer of water cooled down considerably below the freezing-point, and then crystallized suddenly and in a mass. The lower stratum of ice may have subsequently formed itself in precisely the same way, its different age being proved by its different system of cracking. Quite different in look and origin from the cracks here described were the much wider fissures which are formed by the cooling and contraction of the ice subsequent to its perfect congelation*. I found one of these at the place where we were stopping. It ran from thence to the north-east and south-west with little deviation, to the horizon. It had throughout a uniform width of 4 inches, and reached from the upper surface to the water. It was filled with new ice, which gave it the look of a vein or dyke in rock. What added to this resemblance was, that the ice fill-

* Compare *Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. 16. Travels in Siberia, &c. vol. ii. p. 196.

February 13.—We turned off from the western coast (of Baikal) directly across the sea, till we made Posolskoi at the opposite side. There was no snow upon the ice, so that its surface shone as a polished mirror. The regular and steady tread of our horses' feet rang over the wide and dreary waste, interrupted now and then by the shrill sound occasioned by the sledges when they turned round and yielded at the same time to the draught, or *by the duller noise emitted from the ice cracking under the increasing severity of the frost.*

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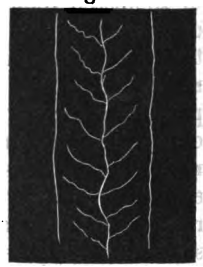
ing the crack was always much whiter than the surrounding; it was, in fact, traversed by fine cracks or flaws in a very regular and remarkable manner. One of these cracks formed a continuous and somewhat waving line, which occupied almost exactly the middle of the vein, and from that proceeded, at an acute angle, an immense number of smaller flaws to each side (fig. 1), just like the lateral ribs of a leaf issuing from the central rib. It seemed rather difficult to give a thorough explanation of this singular mode of fracture; but at least it was evident that the mass in the vein had been subjected to a great strain, owing to the pressure under which the water was frozen, for immoveable ice on both sides had resisted the dilatation which attends the process of congelation.

Fig. 1.



Additional Remark (*April* 1859).—These observations, which were remarkably favoured by the transparency of the water and of the ice of Lake Baikal, show us a primitive sort of ice clearly distinguishable from a later or injected kind, and, again, in the former—

1. *A division into horizontal strata.*—Two of these strata, and of course the most perfect ones, were observed; but more of them would perhaps have been found if the ice had been anatomized by Mr. Tyndall's ingenious proceeding of interior melting. This horizontal fissility is evidently connected with the vertical direction of an optical and crystallographic axis in the ice of unmoved water, which was first pointed out by Sir David Brewster, and is now acknowledged by all opticians.

2. *Divisions of each of the horizontal strata by perpendicular planes into almost hexagonal columns.*—The width of these perpendicular cracks or plane fissures is so small, that they only become visible by the air or the vacuum in their interior and the adjoining solid acting differently upon the light. It is by these same circumstances of their small width and their emptiness, that fissures of this kind differ from those which are formed by the ice cooling beneath the freezing-point, and which are then mostly filled up with the solid product of a new congelation. We have therefore to consider—

3. *These cracks, owing to contraction,* and

4. *Those that are produced in the ice of posterior formation,* viz. in that which is afterwards injected in the previously mentioned (*sub* 3) cracks by contraction. And so it seems that, according to the producing forces, we must distinguish in lake-ice, where formed without disturbance by waves or the like, between—

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A. A horizontal division, mentioned under (1), and due to the perpendicularity of crystallographic axes, and perhaps to compression in a vertical sense.

B. Two kinds of vertical fissures, mentioned under (2) and (4), and arising from the lateral compression mutually exerted by the particles of freezing water. The rending effect of this compression agrees with what, in the case of other substances, is commonly called crushing. And—

C. The broad fissures occasioned by contraction.

I must own that the crushing of ice or its rending by compression seems to me as explicable, or even more so, than the rending of the same by contraction. As a phænomenon the latter is of course more familiar to us, and therefore generally acknowledged; but on theoretical grounds we can neither expect nor should be able to explain its taking place in any mass of which all parts were equally cooled.

Some instances of a dry or wet soil being rent by contraction.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. 215. Travels in Siberia, vol. ii. p. 326.)

March 25, at Kivensk in the valley of the Lena. . . . Towards evening the clouds vanished, and I observed transits of the stars, to determine the geographical position and the magnetic declination. In a few hours the air had again cooled down to 16° R.*; and in the yard where I set my instruments, the ground cracked with a loud report from the rapid contraction which it underwent.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. 247. Travels in Siberia, &c. vol. ii. p. 368.)

April 8, near Yakutsk.—The Lena was now divided into a number of arms; and between the ice of these, lay islands with snow heaped high above, while their sides, from 15 to 20 feet high, were quite black. These islands consist of fine mud with fragments of willow-stems and roots, as if swept together by inundations of the stream; but the unparalleled frost which their naked sides have to endure every year had cracked them vertically, and divided them into such rude pillars that they reminded one of the limestone rocks of the upper valley. The

* The same day I noted the temperature of the air as follows:—

h	m			
At 1	15	P.M.	. . . .	— 3·5 R.
4	45	„	. . . .	0·0 „
5	0	„	. . . .	— 4·5 „
7	30	„	. . . .	—16·6 „

See *Reise um die Erde, u. s. w., Physikalische Abtheilung*, vol. i. p. 372.

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narrow clefts were filled from above with ice, which now looked like shining veins in a black rock*.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. i. p. 648. Travels in Siberia, &c. vol. ii. p. 25.)

December 9, at Obdorsk; lat. 66° 31′, long. 64° 22′ east from Paris.—Hills separated by narrow glens form the right bank of the river Polni. . . . . Here it was not the action of water which gave these hills their remarkable outlines, but the frost, which, causing cracks in the earth of great depth, frequently separates masses which resemble basaltic columns of colossal size. The water which runs down to the river along these tracks, on the melting of the snow, wears away only the outer edges, for at a little depth the ground here remains perpetually frozen.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. i. p. 690. Travels in Siberia, &c. vol. ii. p. 65.)

December 12, near Obdorsk. . . . . With a perfectly clear sky, and the thermometer at −27°·5 R., we set off at 11^{h} 18^{m} A.M., just as the sun was rising from the houses on the ridge into the valley. At first we travelled on the ice of the Polni, between hills deeply cracked with frost.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. 159. Travels in Siberia, &c. vol. ii. p. 265.)

February 22, on a Transbaikalian steppe. . . . . In many places the ground was cracked by the frost, and the deep clefts crossed one another in many different directions. I had seen cracks of the same kind in the snowless valley between Troitsko Savsk and Kiakhta, and also at Obdorsk on the driest parts of the elevated banks of the Obi.

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* As for the frost alluded to in this passage, see Travels in Siberia, vol. ii. p. 368 (*Reise um die Erde, Histor. Abth.* vol. ii. p. 252):—

. . . . . I found the constant temperature of the ground and the mean temperature of the air to be both nearly −6° R. . . . . A degree of cold exceeding −40° R. takes place in Yakutsk every year between the 17th of December and the 18th of February, and most frequently in the first three weeks of January. . . . . In the present year (1829) the cold was somewhat severer; on the 25th of January it reached its maximum with −46°·4 R. . . . . In ordinary years the mean temperature of two months is under −32° R., so that mercury is in this place a solid body for one-sixth of the year.

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Peculiar influence of sun-rays upon snow, when they cannot heat it up to the temperature of melting.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. i. p. 704. Travels in Siberia &c., vol. ii. p. 79.)

December 13. On the Obdorian Mountains; lat. 67° 13′, long. 64° 39′ east from Paris, at about 1700 Par. feet (1812 English) above the level of the sea, the air being very dry, and its temperature varying from −26° to −30° R. . . . . The inclined strata (of hornblende slate) were quite bare, and it was only on their eastern borders that snow had accumulated into little hillocks. . . . . It was curious to observe how the perpendicular surface of the heaps of snow behind the rocks had become changed in every instance into hard and mirror-like ice; for this could not have been effected by an ordinary thawing. It is true that the sunbeams, which (in this spot and at this time of the year) played but horizontally over the plain, fell perpendicularly on these walls of snow; but as their intensity was weakened by their passing through the densest strata of the air, they would surely not have elevated the temperature of a substance less fusible than ice from −28° R. to 0° R. The undoubted evaporation which it undergoes even at the lowest temperatures, seems also to prove a continuous liquefaction of its surface; and as this takes place when the surrounding bodies are far from being warmed up to *zero* (of Reaumur), it seems that the extreme surface of ice appropriates to itself the heat of liquefaction, not according to the law of radiation, but perhaps to that of a chemical affinity.

Additional remark (*April* 1859).—I am well aware that this question is connected with the more general one—whether the evaporation of any solid body takes place without a liquefaction of the particles which form its surface? The just-mentioned phænomenon seems to answer it in the negative.

Wonderful transformations which snow-crystals were seen to undergo from causes still to be explained.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. [illegible] Travels in Siberia, &c. vol. ii. p. 501.)

May 13. Lat. 60° 40′, long. 138° 57′ east from Paris, [illegible] 2580 Par. feet (2749 English) above the sea.—I had begun, immediately after noon to measure solar altitudes, when a number of light clouds began to form and then to be driven [illegible] by the west wind. The air cooled down (from about +3° R.) to +1° R., and snow fell for sixteen minutes; then the clouds dissolved again, the evening became clear, and the cold increased in the

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night to −5° R. I have never seen snow in more perfect and more variously-formed crystals than during this short and sudden shower. Each grain fell single, and among the few which settled on the glass or on the metal of my instruments, I could distinguish six different forms. Doubtless many more remained unobserved, for my attention was drawn in the mean time to a more wonderful and quite novel phænomenon. Many of the crystals began to melt the instant they touched a solid body, and some, as it seemed to me, melted while still falling through the air; but in the next moment this was followed always by a new congelation, the grain of snow assuming, not its previous form, but another more complex. Thus, for instance, the most simple crystals which I observed today, consisted of six thin

Fig. 2*a*.

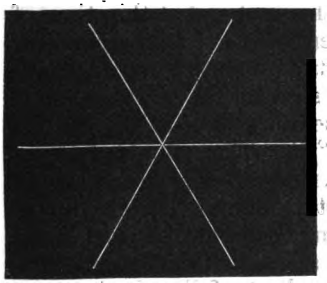


Fig. 2*b*.

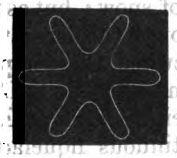


Fig. 2*c*.

needles of ice, which adhered to each other like the diagonals of a regular hexagon (fig. 2 *a*). When melting, each single ray of this star contracted into a thicker cylinder of water, having about half of its former length (fig. 2 *b*); but after a few moments these cylinders were seen to congeal again, and changed thereby into broader plates, sharpened at their outer edges by two planes of a regular hexagonal prism. The whole crystal became thus again an hexagonal star, but with broader and shorter rays than it had before.

Fig. 3*b*.

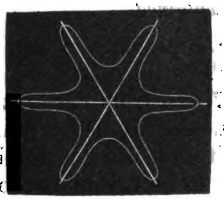
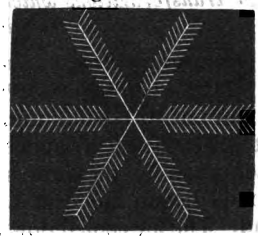


Fig. 3*c*.



Other crystals, which had in the beginning such flat and broad rays (fig. 2 *c*), changed these by melting into feathered ones

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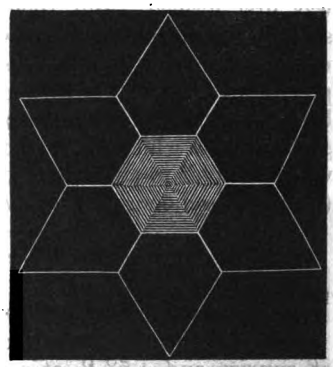
(fig. 3 *c*), because on their liquefaction there remained only the middle of each plate, like an icy needle, in the water, until, the new congelation ensuing, a number of needles ran at each side out of this rib at angles of 60 degrees.

Some of the stars were feathered in the beginning, but only at the outer half of their rays. I did not see any change take place in them, nor did this happen with some other more complicated forms. Thus I observed among others a small and continuous hexagonal plate, with simple rays issuing like diagonals out of its angles; but then each adjoining pair of these rays was still connected by a couple of needles which met at an angle of 60 degrees (fig. 4).

Fig. 4.



But these complicated forms were comparatively rare; and those transformed under my eyes were so predominant, and presented a spectacle so full of motion, that at last I could hardly help comparing them with living beings. In fact, it is only in the case of such that we are accustomed to witness changes so mysterious without inquiring after the forces that produce them. We got, however, a partial explanation of this phænomenon by remarking that the outer parts of the snow-crystal, which were the first to melt, borrowed their warmth of liquefaction from the parts that remained solid, and thereby cooled these below the point of congelation. The newly-formed water could then freeze again by its collecting round this cold ice, and by its offering at the same time a smaller surface* to the air whose temperature had melted the crystal. This water then assumed in freezing a more complicated form, because the remainder of the old crystal exerted in it a greater variety of attraction than that which occurs in a wholly liquid drop. Perhaps all complicated forms of snow result from the simple one by melting and freezing again in this way, a process which they must then undergo during their fall through the air; and here this hypothesis seemed somewhat confirmed by the complicated crystals being always of less diameter than the simple ones.

Additional remark (*April* 1859).—I have sometimes watched the snow-crystals which fell at Berlin when the temperature of the air was a little higher than the freezing-point, but till now

* Viz. the *curved* surface of a single, or of six connected drops.

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without seeing again the phænomenon just mentioned. We may suppose either that these observations were still too rare to present some one of those neglected and apparently trifling circumstances that are requisite for the phænomenon in question, or that this depended also on the spot where I made my first observation having been at a considerable elevation, and consequently not far from the atmospheric stratum where the snow was first formed. But then, as to the explanation of the observed metamorphosis of snow, I think it might have some connexion with the equally obscure property of some chemical precipitates, which, like carbonate of lime, according to M. Ehrenberg, consist, when first consolidated, of regularly arranged solid globules, and which are then changed, "all of a sudden and quite wonderfully," to aggregates of true crystals of microscopic size. (Confer Ehrenberg in *Abhandlungen der Berliner Akademie*, 1840.)

Sunbeams seeming to melt the under surface of the ice of a river.

(*Reise um die Erde, u. s. w., Histor. Abth.* vol. ii. p. 407. Travels in Siberia, &c. vol. ii. p. 513.)

May 15, the temperature of the air varying between -5° R. at sunrise and $+5^{\circ}$ R. at noon; and the elevation being 950 Par. feet (1012 English) above the sea.—Wherever we rode today along the river Arka, or across it from one side of the valley to the other, the effects of the rapid thaw were visible. The stream was quite free in the middle; and at the sides the thickness of the ice, though it varied considerably, was everywhere much diminished. It was very smooth and transparent, and I saw beneath it an astonishing quantity of flattened air-bubbles close together, and almost collected into a continuous stratum. Where the ice was thinnest they were a foot in diameter; and a loud whizzing was heard in such places under the feet of the reindeer as they broke through the thin covering of ice. But the bubbles were always smaller where the ice was strong, evidently because the air was frozen in during the winter, and now, as the thaw advanced, it was liberated and collected at the surface of the water. This proves directly that the icy covering here was melted, not from above downwards, but in the opposite direction. It might perhaps be (but it was highly improbable) that the scarcely warmer water from the middle of the stream which was now open, had got to the banks* and attacked the ice from below, or which is much more likely, that the sun's rays passed inoperative through part of the transparent ice, but developed and gave out their heat near its under side and when they fell on the surface of the water.

* This event seems the more improbable as the river was shallow and moved rapidly in a stony bed.

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LXVIII. *Stereoscopic representation of Print as it appears when viewed with both eyes through Double-refracting Spar.* By Prof. H. W. Dove*.

IN the Report of the Academy, 1858, p. 315, and Pogg. *Ann.* vol. civ. p. 329, I have stated that if a plane drawing be regarded with both eyes through a crystal of calc-spar, one image appears elevated considerably above the other, while if it be regarded with one eye, the two images appear to lie in the same plane. As the reason for the elevation in the former case is to be sought in the different refraction of the ordinary and extraordinary rays, I came to the conclusion that the phænomenon in the calc-spar would be reproduced stereoscopically if the double refraction were represented by a double impression, the different refraction of the two rays being represented by a shifting of the repeated line towards the first line. The six top lines of the accompanying illustration, when regarded in the stereoscope, display this phænomenon in a striking manner; the last line relates to the following paper.

If, in a stereoscope, the drawing designed for the right eye be substituted for that for the left, and *vice versâ*, the convex relief becomes concave. It is obvious that if it be desired to render this change visible, if, for instance, in the case of a truncated pyramid, we wish to observe the passage of the sectional surface through the base, the change must be so managed that, during the period in which it takes place, both projections may remain in the field of view. This can be effected most simply, if a drawing be regarded with one eye naked and with the other through a reflecting prism, and at the same time be turned through an angle of 180°. I have described this in the Report, 1851, p. 249, and Pogg. *Ann.* vol. lxxxiii. p. 185. If it be desired to apply this principle to an ordinary lens- or prism-stereoscope, it is only necessary to fasten the two drawings to equal rotating circles, and to set them turning by means of cross strings. As the phænomenon announced by me has since been exhibited by many physicists, it is possible that this contrivance also has been thought of. The last modification, as far as I know, has been published by Henry Halske†, who has shown that by means of complicated mechanism for moving both images in a direction parallel to the line uniting the eyes, the motion of the section perpendicularly down upon the plane of the drawing may be made visible. It is, however, easy to effect this by the motion of a single image. To give an idea of this, I have had the first

* From Poggendorff's *Annalen*, No. 4, 1859; communicated by the Author.

† Pogg. *Ann.* vol. c. p. 657.

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lines on Slide II. so printed, that now the odd lines, instead of the even ones, are shifted in. The lines that before appeared in the stereoscope depressed, now appear raised. It is clear that if the repeated lines were made moveable on a slide, the motion perpendicularly down upon the plane of the picture would be immediately obvious.

As the double-refracting power of calc-spar decreases with its increasing temperature, while its rhomboidal form approximates continually to the cube, if this change could be carried very far, the *negatively* double-refracting calc-spar, after passing through the state of single refraction, would become at last *positively* double refracting, while the extraordinary ray, which at the common temperature exhibits a less refraction than the ordinary ray, would then exhibit a greater refraction. The experiment above described explains what would appear with relation to the visible motion of an image seen in a crystal of calc-spar subjected to a continually increasing temperature.

LXIX. *On the Application of the Stereoscope to distinguish Prints from Reprints, or generally Originals from Copies.* By Prof. H. W. Dove.

THE considerable degree to which the lines, in the accompanying illustration appear in the stereoscope to be elevated one above another on a comparatively trifling shifting of the lines themselves towards each other in a horizontal direction, shows that we have hereby a means of rendering strikingly visible the difference between prints not absolutely identical. It is obvious that if the interspaces of the individual words are not absolutely equal in the two printed impressions, those which to the naked eye appear in one plane, will (in the stereoscope) rise like steps one over another. The lowest lines of the two slides have been composed of the same type, without the compositor being told that a difference was required, and yet, although the difference of the distance between the second and third words is imperceptible to the naked eye, in the stereoscope the three words rise like steps one over the other—the first word being the lowest, the second higher, the third highest. While, therefore, a re-impression of the sentence from the same form appears all in one plane, a reprint of the same sentence, even though from the same office, and though the greatest care has been taken to preserve the resemblance, will present a perceptible difference. Whether, also, in a new edition only the title has been changed, can be easily determined in the same manner.

What has been said of printing applies of course to copies

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generally. In the imitation of paper money the criterion of comparison has always been the difference of form. The process above suggested affords a much surer test. If, for instance, a bank note and its copy be laid side by side in the stereoscope, a difference in the position of the words, invisible to the naked eye, becomes immediately obvious, as above mentioned, by an apparent projection out of the plane of the paper. By this means, also, a simple and effectual method is obtained of recognizing a copy of a print or drawing as such. The publication of the process has, it is true, the disadvantage of putting into the hands of those who design to forge such copies, the means of testing, by the stereoscope, how closely their work resembles the original; but the difficulty, even with this assistance, of preserving a resemblance bordering on identity is so great, that it would rather have the effect of a warning, since the hope of succeeding in making an exact copy is rendered so remote.

The effect of dampness on paper can be ascertained by moistening one of two proofs of the same sentence placed together in the stereoscope. Should there, on the contrary, be a difference between two proofs of the same sentence due to unequal dryness, they may be made to resemble by wetting both. The influence of temperature—on copper-plates, for instance—may be detected in the same manner, the stereoscope thus playing the part of hygrometer and thermometer.

For the purpose above explained, either the stereoscope should have no bottom, so that it may be placed immediately on the papers to be compared, or, in the place of the four perpendicular dark edges of the table at the bottom, slits should be cut, through which long strips of the proofs to be compared may be drawn, and examined together piece by piece.

If Slides I. and II. be placed at the same time in an ordinary Wheatstone's mirror-stereoscope, one on the right side, the other on the left, the alternate projection of the repeated lines is obvious and is of such a nature that the lines which in the one image are those that project, are those which in the other appear depressed: the letters, however, here appear placed like type. Those who find a difficulty in reading letters so placed, may make use of a Wheatstone's mirror-stereoscope, together with my prism-stereoscope with two prisms (Pogg. *Ann.* vol. lxxxiii. p. 186, No. 4), which Moigno has called the Stereoscope *à réflexion totale*, Wheatstone the *Pseudoscope*. In order to detect the dissimilar parts of large plates, the Wheatstone mirror-stereoscope must be so adapted as to admit of plates of whatever size may be desired. This is easily effected by fastening the reflectors, not between two boards, but to a single one, and by leaving only the under groove for the support of the object, the upper groove being

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omitted. For the purpose of reversing the position of the letters which appear placed like type, a prism-stereoscope may, once for all, be added to a stereoscope of the above description. If it be required to test the identity of two sentences consisting of the same words set up in type, or of two copper-plates before printing, the stereoscope above described is applied at once to the plates, which are laid side by side. In applying the mirror-stereoscope, it is advisable to add two magnifying glasses. As, in this manner, means are afforded to the physicist of testing the equality of the distances between lines, so also the circumstance that in a well-forged bank note the most striking differences immediately show themselves may be cited to justify the application of the stereoscope to the detection of false paper money. It will then be advisable for the Government, if such a note be discovered to be forged, to publish the result of the stereoscopic analysis as an infallible means of detection. If several plates are employed in the preparation of the genuine paper, these must of course be treated as distinct originals. A copy which only presents the slight difference which results from the unequal expansion of the paper, can by the above method be distinguished from one absolutely identical.

LXX. *On Vibrations produced by an Electric Current.* By JOHN TYNDALL, *F.R.S.*

THE last Number of the Philosophical Magazine contains an interesting note from Prof. Forbes on certain Vibrations produced by Electricity. Without knowing that Prof. Forbes had written anything upon the subject, I had in a former Number of the Philosophical Magazine appended a short note to a paper by Mr. Gore, in which I had referred the rotation observed in his experiment to the *heat* produced by the electricity. Prof. Forbes, if I understand him aright, considers the action due to a repulsive power possessed by the electric current itself, and considers that a similar repulsive power, on the part of a current of heat, explains the Trevelyan experiment.

Were this the case, I think it will be agreed that a point altogether new, and of very great importance regarding both heat and electricity, would be established. But, as far as my experiments go, they do not countenance this notion. Many years ago, Page excited Trevelyan's sounds by electricity; and it is now, I think, nearly six years since I made the experiment myself before the audience of the Royal Institution. I then found that, by continuing the action with the voltaic current for a sufficient time, the vibrations were continued *even after the*

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current had been interrupted—doubtless in consequence of the heat which the current had excited.

The manner in which I conceive the current to act will be best understood if we suppose a flat rocker to rest upon two pointed wires, coming alternately into contact with them as it oscillates. Starting the rocker by the hand, let the current be established; it passes between the rocker and the wire on which it chances to rest at the moment the circuit is made; encounters resistance, heats the wire, and expands it; it also produces a *nipple* on the rocker itself, which is tilted up in consequence, and comes into contact with the other point. The current passes through the new channel thus opened to it, heats and expands as before, the rocker being again tilted. Every time the rocker comes into contact with the wire, the heating agent is there, ready to lift it; and thus the vibrations are rendered permanent.

With regard to the method of cooling adopted by Professor Forbes, I should not conclude that it would suspend the phænomenon. The interposition of water does not materially alter the resistance, and cannot abstract the heat with sufficient speed to prevent the expansion. Everybody knows that the electric light can be produced under water. The experiment with the carbon is no more than I should expect; for not only is the carbon lifted by its own expansion, due to the intense heat developed in this substance by electricity, but the pores of the substance are also charged with air and gases, which must expand under the circumstances with almost explosive force. But I think the experiment with the bismuth, where Prof. Forbes finds that a current fifteen times the strength of that used with the carbon is unable to produce vibrations, presents a formidable obstacle to the hypothesis that the effect is due to any repulsive power inherent in the current itself. Prof. Forbes refers this anomalous result to a "quelling power" possessed by the bismuth. The quelling power consists, I imagine, in the fact that bismuth is both a friable substance and has a very low melting-point. The heat that produces nipples on more refractory metals is sufficient to *fuse* it; and hence its inability to produce the effect. Nevertheless, by properly arranging the strength of the current, it is quite easy to obtain vibrations with bismuth also.

In support of the above opinion, I would say that energetic vibrations are obtained with the refractory metals, and the reverse with the fusible ones. The alloy which is technically called *fusible metal,* exercises a quelling power far superior to bismuth; and the want of localization of the heat at the surface, so detrimental to the success of the Trevelyan experiment, is apparent here also in the deportment of silver. Other circumstances

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besides the melting-point come in all probability into play; but it appears to me that this is the most important.

I merely make these remarks, so that my views may be corrected if they are wrong; if they are right, I may, by publishing them, save the time of those experimenters who might be tempted to seek from the subject results which it cannot give.

Royal Institution, May 1859.

LXXI. *Notice of a New Method of causing a Vibration of the Air contained in a Tube open at both ends.* By P. L. Rijke, *Professor of Natural Philosophy at the University of Leyden**.

1. My first experiments were made with a glass tube 0·8 metre long, having a diameter of 37 millims. at the top and 30 millims. at the bottom. Inside, at the distance of 0·2 metre from the latter extremity, I had placed a disc of wire-gauze, about 50 millims. in diameter, with its edges turned back, so that the pressure they exerted on the sides of the tube sufficed to keep the disc at the desired height. The gauze was made of iron wire of 0·2 millim. diameter, and had about 81 meshes to the square centimetre.

The apparatus being thus prepared, it was only necessary to raise the wire-gauze to a red heat by means of a spirit or hydrogen lamp, and then to extinguish or remove the lamp, in order a few moments afterwards to produce a sound. The sound produced was nearly the fundamental note of the tube. It was loud, but only lasted for a few seconds.

2. When, instead of a single disc, several were placed in the tube, the sound lasted longer.

3. The sound ceased immediately the top of the tube was closed, showing that the presence of an ascending current of air is one of the conditions of the phænomenon. For this reason the number of discs must not be excessively multiplied, so as to retard the motion of the air in the tube more than to a certain degree.

4. The experiment is equally successful, if the disc be heated by means of a carbonic oxide flame. I prepared this gas by acting with Nordhausen sulphuric acid upon oxalic acid. As an excess of precaution, I dried the gas before burning it.

This experiment proves that the presence of vapour of water is not one of the necessary conditions of the phænomenon.

5. Tubes of dimensions different from the one described above might be successfully employed, but they must not be less than 0·2 metre long.

In order that the sound may have its maximum intensity, the

* Communicated by the Author.

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distance of the disc from the lower end of the tube should be one-fourth of its entire length.

6. As to the explanation of the phænomenon, that, I think, is not hard to find. In warming the wire-gauze, the temperature of the sides of the tube is raised also. If the lamp be then withdrawn, an ascending current of air is established, which, passing through the meshes of the wire-gauze, is necessarily heated, and in consequence dilates. To this dilatation immediately succeeds a contraction, due to the cooling effect of the sides of the tube. It is to these successive dilatations and contractions, in my opinion, that we must attribute the production of the sounds the origin of which is in question.

It is obvious that the presence of the heated disc alone would suffice to produce an ascending current of air, and that the elevation of the temperature of the sides of the tube is rather injurious than otherwise to the success of the experiment. In fact, that which is of the greatest importance is, that *the difference of temperature between the air which rises in the tube and the meshes of the wire-gauze should be the greatest possible.* Several considerations support this view of the case. For instance, we have seen (1) that the tube only began to sound after a lapse of some instants from the time when the lamp was extinguished; now it is evident that, during this interval, the air warmed by the flame of the lamp, as well as the products of combustion, must have been replaced by a column of air of a much lower temperature. If the sound only lasts some seconds, that is because the gauze is rapidly cooled by the ascending column of air; and if an augmented number of discs gives an increased duration to the sound, that is because the presence of a greater number of discs, by diminishing the rapidity of the air-current, diminishes also the rapidity of cooling of the first disc.

7. If the explanation I have just given is correct, it is evident I ought to obtain a permanent sound by raising the wire-gauze disc to a red heat by means of a galvanic current. The first experiments undertaken to verify this result were unsuccessful. I had not sufficiently considered the cooling effect of the moving air on the wire-gauze, and the degree of intensity which the galvanic current should have, in order that its calorific effect might compensate for this refrigeration. It was only by making use of thirty of Grove's cells combined so as to constitute a battery of six cells, that I had the satisfaction of obtaining a permanent sound. It was so loud that it could be easily heard two or three rooms from the laboratory where the experiment was being performed. Nevertheless, the wire-gauze was not raised all over to a red heat.

The number of vibrations of the sound produced was deter-

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mined by means of a sonometer, provided with a metallic cord, tuned to a pitch of M. Marloye which gives Ut_3, corresponding to 256 entire vibrations. The number of vibrations of the sound produced by the galvanic current, has been found to be 226. The tube I made use of for this experiment was the one described above. The tube being cool, when a current of air is blown against the edge of one of its openings, a sound is obtained corresponding to 208 vibrations.

8. If by any means the intensity of the galvanic current be gradually diminished, a period at last comes when the sound ceases. It may then be immediately reproduced by introducing into the upper extremity of the tube another disc. This experiment succeeds whatever be the distance between this new disc and that traversed by the galvanic current. This reproduction of the sound is easily explained by what we have seen above (6).

9. The sound may also be reproduced, but not so as to be permanent, by interrupting the galvanic current, and suffering the tube to cool till it has acquired the temperature of the surrounding bodies. If the current be then re-established, the tube immediately emits a sound. This sound commences loud, but at once begins to diminish, and finally dies out. The cooling of the tube is much favoured by exposing it to a current of cold air. This experiment also seems to me not to require explanation (6).

10. There is also another very simple way of reproducing the sound, that is, by closing for some time either of the ends of the tube. The current of air being arrested, the temperature of the disc is considerably augmented. In each experiment I remarked that it had become red-hot throughout a considerable part of its extent. On opening the end of the tube, a very loud sound is heard, which, however, only lasts some seconds.

11. The sounds (8), (9), and (10) were the same as those I had produced before, and corresponded to 226 vibrations.

12. M. Bosscha, on repeating some of my experiments, has observed that sometimes a sound is produced the instant the flame of the lamp is applied to the disc. That this experiment may succeed, it is necessary that the flame be held at some distance from the wire-gauze disc. This sound also only lasts for a short time. It is nearly an octave above the fundamental note of the tube, and from time to time it seems to have a tendency to rise.

We found in experimenting with the carbonic oxide flame, that when the sound was at the loudest, the flame detached itself from the tube from which the gas issued, and formed a luminous cloud below the disc, the borders of which were animated with a very visible trembling motion; the distance

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between this cloud, and the extremity of the slender tube whence the gas was disengaged, might be increased to 10 millims.

13. It is evident that in the experiment of M. Bosscha, the production of the sound must be attributed to the cooling and consequent contraction of the heated gas, by the wire-gauze disc through which it passes. This experiment may be considered the inverse of those that precede.

Leyden, 27th April, 1859.

LXXII. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D., F.C.S., Teacher of Physical Science in the Cheltenham College.*

[Continued from p. 280.]

LIMPRICHT* made the observation that carbonic acid passed over metallic copper at a dull red heat, was partially reduced to carbonic oxide, and that hence the usual methods of analysing nitrogenous substances in which the use of metallic copper was involved, were liable to error. This observation led to some experiments by Lautemann†, who confirms the statement as far as concerns the use of the porous spongy copper obtained by reducing granulated oxide of copper. In one case he found that when carbonic acid was passed over a layer of granulated oxide of copper 30 centims. long, about $1\frac{1}{2}$ per cent. was reduced to carbonic oxide. But the use of freshly-reduced copper turnings is not liable to this error. In a special case, with a layer the same in length as in the previous case, the amount of carbonic oxide formed was only 0·05 per cent. of the carbonic acid passed over—an amount which may safely be neglected. The error may also be obviated by placing a layer of oxide of copper in front of the metallic copper.

In connexion with this subject, Perrot‡ found that *pure* copper turnings do not decompose carbonic acid; but if the copper be not quite pure, and contains iron or zinc, part of the carbonic acid is reduced with formation of carbonic oxide.

In a communication on chlorous acid, Schiel§ states that by observing a few precautions its preparation may be effected without danger, and almost as conveniently as that of chlorine. The method he uses is that of Millon; and the proportions are 2 parts chlorate of potash, 3 parts nitric acid of 1·30 sp. gr., 0·6 to 0·8 parts cane-sugar, and 3 to 4 parts of water. It is necessary that the chlorate of potash and the nitric acid be pure, although the

* Liebig's *Annalen*, October 1858. † Ibid. March 1859.
‡ Ibid. March 1859. § Ibid. February 1858, and March 1859.

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presence of a trace of sulphuric acid in the latter is not injurious. The mixture is heated in the water-bath at a temperature of about 60° C.; and the flasks are filled, so that when the expansion has taken place the neck is about half full. By this method Schiel has prepared the acid on a scale twenty times as large as that indicated in the books.

Water absorbs ten or twelve times its volume of chlorous acid; the solution is of a deep yellowish-red colour, and may be kept for some time without decomposition. It is very valuable as a deodorizer and disinfectant; its oxidizing power, as compared with that of chlorine, is as 4 : 1; it dissolves amorphous phosphorus almost instantaneously. The most interesting salt is the chlorite of lead, $PbO\,ClO^3$; this is prepared by nearly neutralizing a solution of the acid with milk of lime, filtering, precipitating the filtrate while warm with nitrate of lead, and washing out the filtrate with distilled water. Mixed with sulphur and exposed to friction, it explodes: it is also remarkable for exploding at the temperature of boiling water. Under certain circumstances it combines with chloride of lead to a double salt of the composition $2PbO\,ClO^3$, PbCl, which forms pale yellow acicular crystals. Chlorous acid seems to exercise a curious action on organic substances, more especially on urea, albumen, and uric acid; with the study of these the author is engaged.

In continuation of his investigations into the equivalent weights of the elements, Dumas* has pointed out further interesting relations.

Among the substances investigated, twenty-two have equivalents which are exact multiples by whole numbers of that of hydrogen taken as unity:—oxygen, 8; sulphur, 16; selenium, 40; tellurium, 64; nitrogen, 14; phosphorus, 31; arsenic, 75; antimony, 122; bismuth, 214; fluorine, 19; bromine, 80; iodine, 127; carbon, 6; silicon, 14; molybdenum, 48; tungsten, 92; lithium, 7; sodium, 23; calcium, 20; iron, 28; cadmium, 56; tin, 59.

Seven have equivalents which are multiples of half the equivalent of hydrogen:—chlorine, 35·5; magnesium, 12·5; manganese, 27·5; barium, 68·5; nickel, 29·5; cobalt, 29·5; lead, 103·5.

Three have equivalents which are multiples of a quarter the equivalent of hydrogen:—aluminium, 13·75; strontium, 43·75; zinc, 32·75.

The result of all experiments to revise the equivalents has been to approximate the numbers more and more closely to those given in the Tables.

* Liebig's *Annalen*, December 1858. *Comptes Rendus*, vol. xlvi. p. 951.

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Among the relations which exist among these numbers, the following is remarkable:—

N 14	P 31	As 75	Sb 122
Fl 19	Cl 35·5	Br 80	I 127.

It will be observed that the equivalents of nitrogen and antimony show the same difference (108) as those of fluorine and iodine: the equivalents of nitrogen and arsenic show the same difference (61) as those of fluorine and bromine. The equivalents of the lower horizontal line are all greater by 5 than those of the upper, with the exception of those of phosphorus and chlorine, where the difference is 4·5. Dumas made many experiments to discover a possible source of error in previous determinations of the equivalent of phosphorus; but the result was in all cases to confirm the number (31) obtained by Schrötter.

In a subsequent paper, Dumas* continues his discussion of the relations between the equivalents of certain natural families of the elements, and also points out that the same obtains in the comparison of compound radicals. Thus in the following two series,

Mg 12·25	Ca 20	Sr 43·75	Ba 68·5	Pb 103·5
O 8	S 16	Se 40	Te 64	Os 99·5

the common difference is nearly 4. In the two series

NH^4 18	$NH^3.C^2H^3$ 32	$NH^3.C^4H^5$ 46	$NH^3.C^6H^7$ 60
C^2H^3 15	C^4H^5 29	C^6H^7 43	C^8H^9 57

the common difference is 3.

He enters into lengthened considerations as to how far chemical elements are to be considered as really simple, or merely as being undecomposable, the result of which may be thus stated. The decomposition of substances existing in the three kingdoms of nature leads to the knowledge of certain radicals, which may be arranged in certain natural families. The families, not only of the organic (decomposable), but also of the inorganic (undecomposable) radicals exhibit undeniable analogies. With the means at present at our command, the inorganic radicals cannot further be decomposed, even if they are at all decomposable. But from their analogies with the decomposable radicals, it is still an open question whether they are not also decomposable.

In an analysis of a meteoric stone which fell at Kaba in Hungary, Wöhler† made the interesting observation that it contained

* *Comptes Rendus*, vol. xlvii. p. 1026. Liebig's *Annalen*, March 1859.
† Liebig's *Annalen*, March 1859.

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a substance of organic origin. The stone had the usual composition of a meteorite, but contained in addition a certain quantity of free carbon, and, further, a small quantity of a carbonaceous, readily fusible, and partially volatile substance, soluble in alcohol. This substance had most analogy with the fossil waxes, as ozokerite; but the quantity was too small to permit of a quantitative investigation.

Kawalier obtained in 1852, from the leaves of the Arbutus, a crystalline bitter principle which he named *arbutine.* He ascribed to it the formula $C^{32} H^{22} O^{19}$; he found that under the influence of emulsine it was resolved into grape-sugar and another crystallizable substance, *arctuvine.* This substance has recently been examined by Strecker*, who assigns to it another formula, and shows that the arctuvine of Kawalier is identical with hydrochinone obtained by Wöhler from kinic acid. When arbutine is boiled with dilute sulphuric acid, it is resolved into grape-sugar and hydrochinone. The latter substance is obtained by agitating with ether the acid aqueous solution in which it is formed; on evaporation, the etherial solution deposits hydrochinone in colourless crystals which are soluble in alcohol and water. Its physical properties, composition, and decompositions leave no doubt as to its identity with the substance discovered by Wöhler.

Strecker expresses the formula of arbutine by $C^{24} H^{16} O^{14}$; its resolution into hydrochinone and grape-sugar takes place in accordance with the equation

$$\underset{\text{Arbutine.}}{C^{24} H^{16} O^{14}} + 2HO = \underset{\text{Hydrochinone.}}{C^{12} H^{6} O^{4}} + \underset{\text{Glucose.}}{C^{12} H^{12} O^{12}}.$$

Arbutine thus corresponds to salicine, which under similar circumstances experiences an analogous change.

$$\underset{\text{Salicine.}}{C^{26} H^{18} O^{14}} + 2HO = \underset{\text{Saligenine.}}{C^{14} H^{8} O^{4}} + \underset{\text{Glucose.}}{C^{12} H^{12} O^{12}}.$$

Since salicine and arbutine, as well as saligenine and hydrochinone, only differ by $C^{2} H^{2}$, it may be asked whether they are not homologous. A great similarity is evident in the two series.

Arbutine	$C^{24} H^{16} O^{14}$	Salicine . . .	$C^{26} H^{18} O^{14}$
Hydrochinone . . .	$C^{12} H^{6} O^{4}$	Saligenine . . .	$C^{14} H^{8} O^{4}$
Chlorhydrochinone .	$C^{12} H^{5} ClO^{4}$	Chlorsaligenine .	$C^{14} H^{7} ClO^{4}$
Chinone	$C^{12} H^{4} O^{4}$	Hydride of salicyle.	$C^{14} H^{6} O^{4}$
Chlorochinone. . .	$C^{12} H^{3} ClO^{4}$	Chlorosalicylous acid	$C^{14} H^{5} ClO^{4}$

At the same time the analogy between these two groups is not quite so complete as might be expected from true homologues. Thus chinone and hydride of salicyle are very different

* Liebig's *Annalen*, August 1858.

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in their properties, although they are formed under similar circumstances from hydrochinone and saligenine, which correspond to each other.

Under the influence of various reagents—oxygen, chlorine, nitric acid—arbutine undergoes a series of very interesting changes, with the investigation of which Strecker is still engaged, and the results of which will doubtless throw great light on the constitution of this substance.

Zwenger* has published a preliminary notice of an investigation of solanine, which has already yielded some very interesting results.

Solanine, a peculiar alkaloid found in many species of *solanum*, is a feeble base, with scarcely any alkaline reaction, and the salts of which are readily decomposed. In this respect it resembles narcotine. When solanine is boiled with dilute hydrochloric or sulphuric acid, the liquid becomes turbid, and crystals separate, the quantity of which gradually increases. This crystalline precipitate is a compound of the acid used with a newly-formed base, which Zwenger calls *solanidine*. Its salts are not very soluble in water; by recrystallization from absolute alcohol they may be obtained in good crystals. The alcoholic solutions of salts of the base give with ammonia a gelatinous precipitate of the base, which is not soluble in water, but is readily so in alcohol and ether, from which it may easily be crystallized. The crystals may be sublimed without decomposition. It has a strongly alkaline reaction, neutralizes acids, and gives a crystalline double salt with bichloride of platinum. It imparts to sulphuric acid an intense red colour. The most unexpected part of the reaction is, that the product formed at the same time is grape-sugar. The solution from which the sulphate of solanidine is deposited was decomposed by baryta, and the sulphate of baryta filtered off. The filtrate contained grape-sugar with a trace of solanidine, which imparted to it a bitter taste; but by repeated evaporations to dryness and extraction with water, the solanidine was got rid of, and the aqueous solution, evaporated to a syrup, deposited crystals which had all the characteristic properties of grape-sugar. They were found to reduce oxide of copper, and, mixed with yeast, underwent the alcoholic fermentation.

Inasmuch as, besides the new base and grape-sugar, the metamorphosis of solanine yields no other products, it must be regarded as belonging to the class of glucosides—as being, in fact, a hydrate of carbon copulated with solanidine. And this reaction is specially interesting as affording the first instance of

* Liebig's *Annalen*, February 1859.

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a base thus copulated, although probably investigations undertaken in this direction will reveal the existence of others.

According to Geuther*, when sodium-alcohol is heated to the temperature of boiling water in dry carbonic oxide gas, an absorption takes place, and the sodium-alcohol is converted into a white, solid, crystalline mass. The experiment was undertaken in the expectation that it would lead to the formation of propionic acid,

$$C^4 H^5 NaO^2 + 2CO = C^6 H^5 NaO^4.$$

Sodium-alcohol. Propionate of soda.

An acid was indeed formed; but it was *formic*, and not propionic acid. From this Geuther concludes that sodium-alcohol is changed by carbonic oxide into olefiant gas, $C^4 H^4$, and hydrate of soda, NaO HO; and that the formic acid arose from the action of the carbonic oxide on the latter. Geuther was not able to detect the olefiant gas formed.

Wanklyn† also examined this reaction. He found, when pure sodium-alcohol was heated in closed vessels in carbonic oxide and afterwards opened over mercury, that only about one-fifth of the gas was absorbed. This contraction arose partly from the difference between the temperature of the flask at the time it was sealed, and that at which the contraction was observed, and partly from the absorption of the carbonic oxide by the hydrate of soda always contained in sodium-alcohol. By a careful analysis of the residual gas, Wanklyn found that it contained no trace of olefiant gas, and hence he concludes that the formic acid produced in Geuther's experiment must have arisen from the hydrate of soda contained in the sodium-alcohol used.

In continuing his researches on glycol, Wurtz has obtained a body which he considers to be the true *ether of glycol*. When glycol, saturated with hydrochloric acid gas, is heated for some time in a closed vessel, combination takes place with the elimination of water, and formation of a neutral chlorinated body which is a kind of ether,

$$C^4 H^6 O^4 + HCl = C^4 H^5 ClO^2 + H^2 O^2.$$

Glycol. New body.

This Wurtz names *monohydrochloric glycol*. It is a colourless, neutral, soluble liquid, boiling at 128° C. When this substance is treated with potash, it is decomposed with formation of chloride of potassium, and a gas, or rather vapour, which is inflammable and burns like olefiant gas. This is the *oxide of ethylene*,

* Liebig's *Annalen*, January 1859. † Ibid. April 1859.

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$C^4H^4O^2$. Its formation may be thus expressed:—

$$C^4H^5ClO^2 + KO = C^4H^4O^2 + KCl.$$

Monohydrochloric glycol. Oxide of ethylene.

The formula was determined both by analysis and from its vapour-density, which agrees with the calculated numbers. It is isomeric with aldehyde, with which it has many points of resemblance and also of difference. It boils at 13°·5. Aldehyde boils at 21°. Like aldehyde, it forms definite crystalline compounds with alkaline bisulphite; but, unlike aldehyde, it does not form with ammoniacal ether the crystalline etherial compound characteristic of aldehyde*. Pentachloride of phosphorus acts upon it, forming chloride of ethylene, and oxychloride of phosphorus.

By treating propyle-glycol with hydrochloric acid, and then with potash, Wurtz obtained the oxide of propylene, $C^6H^6O^2$, isomeric with propylic aldehyde, and with acetone.

The relations between the formulæ of aldehyde and of glycol may be thus written:—

$$\left.\begin{matrix} C^4H^3O^2 \\ H \end{matrix}\right\} \qquad C^4H^{4''}.O^2.$$

Aldehyde. Oxide of ethylene.

Wurtz considers the bodies here described to be the true ethers of glycol; for they yield the chlorides, and from the chlorides the glycols themselves, while from the aldehydes the glycols cannot be prepared. It is true that the glycols, by treatment with dehydrating agents, yield aldehydes; but in this case the reaction is very violent, and it is possible that the product at first formed passes into aldehyde by a molecular transformation.

M. Borodine† examined the action of iodide of ethyle on hydrobenzamide and on amarine, in the expectation of obtaining a clue to the constitution of these isomeric bodies. The principal product of the action of iodide of ethyle on the former substance is an oily body, which, when treated by oxide of lead, and purified by animal charcoal, is ultimately obtained in the form

* An analogous relation is found to exist between butyral and butyraldehyde. The former, which occurs among the products of the destructive distillation of butyrate of lime, boils at 95° C. It has the formula of butyric aldehyde, $C^8H^8O^2$, and has all the properties of an aldehyde, with the exception of that of forming a crystalline compound with ammonia. Butyraldehyde, which is formed by the oxidation of certain animal substances, and boils at 68° to 75° C., appears to be the true aldehyde, for it forms the compound with ammonia.—E. A.

† Liebig's *Annalen*, April 1859.

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of a soft viscous substance, readily soluble in alcohol, but scarcely so in water. It has strongly basic properties, and forms salts, which, however, do not crystallize. Analysis gave for this body the formula $C^{50}H^{28}N^2O^2$. Borodine views it as an ammonium-base—as being derived from a double atom of oxide of ammonium, in which 6 atoms of hydrogen have been replaced by 3 equivs. of the biatomic group $C^{14}H^6$, and the other two by 2 equivs. of ethyle.

$$\left.\begin{matrix} NH^4 \\ NH^4 \end{matrix}\right\} O \qquad \begin{matrix} N\left\{\begin{matrix} C^4\ H^5 \\ C^{14}H^{6''} \\ C^{14}H^{6''} \end{matrix}\right\}O \\ N\left\{\begin{matrix} C^{14}H^{6''} \\ C^4\ H^5 \end{matrix}\right\}O \end{matrix}.$$

He views hydrobenzamide as a double atom of ammonia, in which all the hydrogen is replaced by 3 equivs. of the biatomic group $C^{14}H^6$. $N^2\left\{\begin{matrix} C^{14}H^{6''} \\ C^{14}H^{6''} \\ C^{14}H^{6''} \end{matrix}\right.$.

Amarine, treated with iodide of ethyle, furnishes, besides hydriodate of amarine, the hydriodic acid compound of a base, diethylamarine. The action is thus :—

$$2(C^{42}H^{18}N^2) + 2C^4H^5I = C^{42}H^{18}N^2HI + C^{42}H^{16}(C^4H^5)^2N^2, HI$$

Amarine. | Iodide of ethyle. | Hydriodate of amarine. | Hydriodate of diethylamarine.

The hydriodate of diethylamarine crystallizes well. By treatment with potash it is decomposed into iodide of potassium and the base diethylamarine, $C^{42}H^{16}(C^4H^5)N^2$, a substance crystallizing in acute rhombic prisms. The sulphate and nitrate of diethylamarine do not crystallize. By treating diethylamarine with iodide of ethyle, the hydriodic compound of another base is obtained, whose alcoholic solution, treated by oxide of silver, yields iodide of silver, and the free base which may be obtained in rhombic prisms. This base, again treated by iodide of ethyle, yields further substitution-products. Amarine is regarded as containing 3 equivs. of replaceable hydrogen. He considers it as derived from a molecule of ammonia in which an equivalent of hydrogen is replaced by a substituted ammonium, as $N\left\{\begin{matrix} N(C^{14}H^6)^3H \\ H \\ H \end{matrix}\right.$; and this would explain why 2 atoms of hydrogen are more easily replaced than the third.

The view of the constitution of hydrobenzamide above given finds a support in a recent experiment by Engelhardt*. He

* Liebig's *Annalen*, April 1859.

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found that chlorobenzole, $C^{14} H^{6''} Cl^2$, which is commonly regarded as the chloride of the biatomic radical $C^{14} H^6$, when digested for some months with aqueous ammonia, is converted into hydrobenzamide, the formation of which would be thus expressed:—

$$3(C^{14} H^{6''} Cl^2) + 8NH^3 = N^2(C^{14} H^{6''})^3 + 6NH^4 Cl.$$
Chlorobenzole. Hydrobenzamide.

Harnitzki* examined the action of phosgene gas on aldehyde. When vapours of aldehyde are brought in contact with phosgene gas, a brisk action is set up, with disengagement of hydrochloric acid, and formation of a volatile product whose vapours condense to a liquid, which can be obtained in crystalline lamellæ, melts at 0°, and boils at about 45°. The analytical results, confirmed by a vapour-density determination, lead to the formula $C^4 H^3 Cl$. Its formation would be thus expressed:—

$$C^4 H^4 O^2 + C^2 O^2 Cl^2 = C^4 H^3 Cl + HCl + 2CO^2.$$
Aldehyde. Chlorocarbonic acid. New body.

The new substance, which is named *chloracetene*, is isomeric with chloride of vinyle, or chlorinated ethylene; and their vapour densities are also identical. But in their physical properties the two substances differ materially, and more especially in their action upon water. When chloracetene is placed in water, it assumes a buttery consistence, and then dissolves with decomposition, forming hydrochloric acid and aldehyde,

$$C^4 H^3 Cl + 2HO = C^4 H^4 O^2 + HCl.$$
Chloracetene. Aldehyde.

LXXIII. *Notices respecting New Books.*

A Treatise on Differential Equations. By George Boole, *F.R.S., Professor of Mathematics in the Queen's University, Ireland, Honorary Member of the Cambridge Philosophical Society.* Cambridge: Macmillan and Co. 1859.

PROFESSOR BOOLE has long been known in the mathematical world as one of the most original and profound analysts of the present century. On the subject of differential equations, in particular, his researches (printed in the Philosophical Transactions, Philosophical Magazine, Cambridge Mathematical Journal, &c.) have been of the highest order, and have contributed very greatly to the extension of the science. He has now condescended to the task of writing an elementary treatise on his favourite subject; but, as

* *Répertoire de Chimie*, March 1859.

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might have been expected from such an author, this "Elementary" work abounds in matter which could only have been produced by a mathematician of the highest rank. The nature of the book is clearly stated in the Preface:—"I have endeavoured, in the following Treatise, to convey as complete an account of the present state of knowledge on the subject of differential equations as was consistent with the idea of a work intended primarily for elementary instruction. It was my object, first of all, to meet the wants of those who had no previous acquaintance with the subject; but I also desired not quite to disappoint others who might seek for more advanced information. These distinct, but not inconsistent aims determined the plan of composition. The earlier sections of each chapter contain that kind of matter which has usually been thought suitable for the beginner, while the latter ones are devoted either to an account of recent discovery, or to the discussion of such deeper questions of principle as are likely to present themselves to the reflective student in connexion with the methods and processes of his previous course."

We should have been glad if Professor Boole had entered more fully into the difficulties which occur in the application of what are called "Symbolical" methods, and especially in *Partial* Differential Equations. Considering the very important place held by equations of this class in physical inquiries (those of sound and light, for instance); considering, moreover, that in the application of symbolical methods to the solution of these equations we meet with some of the most interesting and instructive views of the real nature of these methods,—we cannot but regret that more space has not been devoted to this part of the subject by one who is so well qualified to handle it. We would suggest to the author the publication of a supplementary volume appropriated to "Symbolical Methods" alone, and going fully into the difficulties connected with their application; not that we expect a complete solution of all such difficulties even from Professor Boole, but because we feel certain that their collection and comparison would bring into notice their mutual dependence, and diminish the apparent *number* of obstacles to be overcome, though unable to conquer them all. In the meanwhile we thank the author most sincerely for placing in the hands of students and teachers a treatise so incomparably superior to that of 'Hymers,' or any other elementary book on the same subject with which we are acquainted.

In a future edition the author will doubtless correct a number of errata which occur in the solutions to the 'Examples;' but to prevent any embarrassment to students who read the book without a tutor, it would be well to supply a list of these errata to purchasers of the present edition.

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LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 372.]

January 13, 1859.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

THE following communications were read:—

"On the Embryogeny of *Comatula Rosacea* (Linck)." By Prof. Wyville Thomson.

"On the Stratifications in Electrical Discharges, as observed in Torricellian and other Vacua."—Second Communication. By J. P. Gassiot, Esq., V.P.R.S.

The author of this Paper states that he procured several vacuum-tubes from M. Geissler of Bonn, and alludes to the experiments made in similarly constructed tubes by M. Plücker (Phil. Mag. August 1858), but finding it impracticable to ascertain with accuracy the nature of the residual gas, he reluctantly laid them aside. All the vacuum-tubes in which his experiments were made, were prepared by himself or in his presence; as each was exhausted and hermetically sealed, it was marked with a consecutive number; upwards of 100 were thus prepared; many were broken or otherwise destroyed, but the remainder he retains with the original numbers for future reference. The author uses several terms, which he explains: air, hydrogen, oxygen, or nitrogen (mercurial) denote that the vacuum-tube contains vapour of mercury *plus* the air or gas remaining in the tube with which it was filled previous to the introduction of the mercury: he applies the terms outer positive or negative, and inner positive or negative, to denote the character of the discharge from the terminals; conductive and reciprocating denote the peculiar conditions of discharges from an induction apparatus when taken in vacuum-tubes; with a conductive discharge the needle of a galvanometer placed in the circuit will be deflected, as are also the stratifications on the approach of a magnet—they having, as the author has shown in his former communication, a tendency to rotate as a whole round either pole, but in contrary directions; in a reciprocating discharge the stratifications are confused, they are divided or separated by the magnet, and the needle of a galvanometer placed in the circuit is not deflected.

The author explains the condition which the stratified discharge assumes if any air or gas remains or is subsequently introduced into a Torricellian vacuum, and describes what he denominates a white and a blue tongue discharge, which under certain conditions always appears at the negative terminal. In Torricellian vacua, if air or nitrogen is introduced, the stratifications, exclusive of their altered form, exhibit a red colour, while when hydrogen or oxygen is added, they retain the bluish-grey appearance: when the ends of the tubes were punctured by means of an electrical spark from a machine, the air or gas could be admitted so gradually as to occupy two or three

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hours in the experiment, and in this manner the preceding results were obtained.

In the best Torricellian vacua the author has been able to obtain, the stratifications always assumed a long cloud-like appearance; by using ten cells, he on one occasion observed distinct sets of stratifications, one from each terminal, in opposite directions.

From a variety of experiments made in the laboratory of the Royal Institution in temperatures varying from −102° to upwards of +600° Fahr., he obtained the following results:—

When the flame of a spirit-lamp is applied to the discharge in a vacuum-tube, the stratifications, if they are narrow, will become clearer and divided, attaching themselves to the warmer portion of the tube; if a section of the tube is heated, the stratifications in that section will be more separated, becoming closer in the cooler portion.

If heat is applied to a tube which shows the cloud-like stratifications, they will lose their clear distinctness; the deposit from the negative wire appears to be more free, and distinct sparks or discharges are apparent, but none from the positive.

In a Torricellian vacuum from which the mercury was withdrawn, which gave clear cloud-like stratifications, no change could be observed when the temperature was lowered to +32° Fahr.; at a temperature of −102°, all trace of the stratified discharge was destroyed, and in this state the red or heated appearance of the negative wire disappeared, the discharge filling the entire vacuum with a white luminous glow; on the temperature being raised the stratifications reappear. When the mercury in a Torricellian vacuum is boiled, indicating a heat of upwards of +600°, the stratifications are also destroyed; but in this case the mercury as it condenses carries the discharge, becoming a conductor.

When the mercury is frozen the stratifications disappear, and the discharge did not then illuminate the entire length of the tube; on presenting a magnet near the tube, the cloud-like stratifications immediately reappear from the positive terminal, very distinct, but not so clearly separated as when the tube is in its normal state of temperature.

The author being desirous to obtain vacua free from all trace of the vapour of mercury, endeavoured to do so by means of fusible metal, but traces of air were perceptible; he also prepared apparatus for a tin vacuum: in a vacuum obtained by means of oxygen and sodium, very good stratifications were observable. At the suggestion and with the assistance of Dr. Frankland, vacua were obtained by absorbing rarefied carbonic acid by means of caustic potassa. This process is described, and a drawing of the apparatus is given.

In carbonic acid vacua the discharge at first appears in the form of a wavy line; it is strongly affected on the approach of a magnet or by the hand, but does not generally present the stratified appearance; if this be present, it is only near the positive terminal: sometimes in the course of a few minutes, but often not until after several days, stratifications are visible, which, as the carbonic acid becomes absorbed, increase; they subsequently assume a conical

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form, and lastly, the clear cloud-like character of the best Torricellian vacua. Under certain conditions the stratifications disappear, the whole length of the tube being filled with luminosity; when in this state, if the outside of the tube is touched, pungent sparks can be perceived $\frac{1}{4}$th of an inch in length, and the peculiar blue phosphorescent light, that in the ordinary state is perceptible at the negative, is perceptible at both terminals, and a galvanometer shows that the discharge is no longer conductive.

After noticing the difficulty of obtaining in carbonic acid vacuum-tubes precisely the same results, the author describes one experiment in which moisture was purposely introduced; in this tube the stratified discharge was very clear and distinct. He states (and describes the illustrative experiment) that under certain conditions the stratifications entirely disappear, the vacuum insulating the discharge. Carbonic acid vacuum-tubes were prepared, into which *arsenious acid, iodine, bromine, pentachloride of antimony, bichloride* and *bisulphide of carbon* were severally introduced, and the results obtained are described.

In Torricellian vacua the author was necessarily limited in the size of the glass vessels employed, but with carbonic acid this difficulty no longer exists; in one vessel of 7 inches internal diameter, the stratified discharge was observed to fill the entire space; in another, the discharges were made to pass in the middle of the vessel through a small hole in the centre of a glass diaphragm.

After many trials, the author ascertained that if the negative terminal is covered with glass tubing (open at each end) to about $\frac{1}{8}$th of an inch beyond the terminal of the wire, the stratifications are destroyed. In this state the negative discharge appears to issue with considerable force through the orifice; this discharge can be deflected by the magnet, and wherever it impinges, a brilliant blue phosphorescent spot is perceivable, which spot is in a short time sensibly heated. The author remarks that in this experiment there is the *appearance of a direction of a force emanating from the negative*.

In some of the vacuum-tubes beyond the clear cloud-like stratifications, but nearer the negative terminal, several faint striæ can be obtained: on repeating Mr. Grove's experiment (Phil. Mag. July 1858), of allowing the discharge to pass between two metallic points attached to the coil, the author observed that these faint striæ *invariably* disappeared.

Stratifications remarkably sensitive to induction on the approach of the hand were obtained in a glass cylinder of about $4\frac{1}{2}$ inches diameter, in which the wires were hermetically sealed 21 inches apart.

From the absorption of carbonic acid by caustic potassa, not only were vacua obtained far more perfect than by the Torricellian method, but the process can be made so gradual as to occupy several weeks, or even months, thus enabling the experimenter to examine the phenomena of the stratified discharge under a variety of conditions, several of which the author describes; in this manner the *non-transferring condition for the electrical discharge in a vacuum*

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has been experimentally ascertained. The author considers that this confirms the opinion he ventured to offer in his previous paper; for if the pulsations or vibrations of an electrical discharge are greatest in the bright bands and least in the obscure, this system of interference or of pulsations would also account for the entire absence of stratifications when the air or gas is not sufficiently rarefied, as well as when the vacuum becomes nearly perfect, while the gradual change of narrow to cloud-like stratifications is thus satisfactorily explained.

In an additional note to his Paper, the author describes some further experiments, particularly one of moving the vacuum-tube to and fro in a rapid manner, or rotating it in a plane, while the discharges are made, either singly or continuously: in the latter case the stratified discharges are separated, giving the appearance of an illuminated fan or wheel; in the former, only a single discharge is perceptible, taking place in whatever direction the tube may at the instant be placed. The author considers this experiment as confirmatory of his former opinion, that the stratifications *are entirely due to a single disruption of the primary circuit.*

January 20.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communications were read:—

"Second Note on Ozone." By Thomas Andrews, M.D., F.R.S., and P. G. Tait, M.A., F.C.P.S.

Since the publication of their "Note on the Density of Ozone" (Philosophical Magazine for Feb. 1858, p. 146), the authors have been occupied with an extended investigation into the nature and properties of that body. The inquiry having proved more protracted than they anticipated, they have thought it proper to send to the Royal Society a brief notice of some of the more important facts which they have already observed, reserving a description of the methods employed, and of the details of the experiments, for a future communication.

The commonly received statement, that the whole of a given volume of dry oxygen gas contained alone in an hermetically sealed tube can be converted into ozone by the passage of electrical sparks, is erroneous. In repeated trials, with tubes of every form and size, the authors found that not more than $\frac{1}{100}$ part of the oxygen could thus be changed into ozone. A greater effect was, it is true, produced by the silent discharge between fine platina points; but this also had its limit. In order to carry on the process, it is necessary to introduce into the apparatus some substance, such as a solution of iodide of potassium, which has the property of taking up, in the form of oxygen, the ozone as it is produced. After many trials, an apparatus was contrived in the form of a double U, having a solution of iodide of potassium in one end, and a column of fragments of fused chloride of calcium interposed between this solution and the part of the tube where the electrical discharge was passed. The chloride of calcium allowed the ozone to pass, but arrested the vapour of water; so that, while the discharge always took place in

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dry oxygen, the ozone was gradually absorbed. The experiment is not yet finished, but already one-fourth of the gas in a tube of the capacity of 10 cubic centimetres has disappeared. To produce this effect, the discharge from a machine in excellent order has been passed through the tube for twenty-four hours.

When oxygen is thus converted into ozone, a diminution of volume takes place. The greatest contraction occurs with the silent discharge, and amounts to about $\frac{1}{35}$ of the volume of the gas. The passage of sparks has less effect than the silent discharge, and will even destroy a part of the contraction obtained by means of the latter. If the apparatus be exposed for a short time to the temperature of 250° C., so as to destroy the ozone, it will be found that the gas on cooling has recovered exactly its original volume. This observation proves, unequivocally, that if ozone be oxygen in an allotropic condition, its density is greater than that of oxygen. Experiments still in progress indicate that the density of ozone obtained by the electrical discharge must, on the above assumption, be represented by even a higher number than that deduced by the authors from their experiments on ozone prepared by electrolysis.

When mercury is brought into contact with dry oxygen, in which ozone has been formed by the electrical discharge, it loses to a great extent its mobility, and may be made to cover the interior of the tube with a fine reflecting surface resembling that of an ordinary mirror. It is remarkable that this great change in the state of the mercury is not accompanied by any further diminution of the volume of the gas. The apparatus employed by the authors would have enabled them to estimate with certainty a change of volume amounting to $\frac{1}{1200}$ part of the whole. On the contrary, on allowing the apparatus to stand, the gas begins slowly to expand; and in thirty hours, when the ozone reactions have disappeared, the expansion amounts to a little more than one half of the contraction which had previously taken place.

Dry silver, in the state both of leaf and of filings, has the property of entirely destroying ozone, whether prepared by electrolysis or by the electrical machine. If a stream of electrolytic ozone be passed over silver leaf or filings contained in a tube, the metal becomes altered in appearance where the gas comes first into contact with it; but no appreciable increase of weight takes place, however long the experiment may be continued. The volumetric results are similar to those already described in the case of mercury.

Arsenic also destroys dry ozone, but, as it likewise combines with dry oxygen, its separate action on ozone cannot be observed with precision.

Most of the other metals examined, such as gold, platina, iron, zinc, tin, &c., are without action on dry ozone.

Iodine, brought into contact with oxygen contracted by the electric discharge, instantly destroys the ozone reactions, and a yellowish solid is formed: no change of volume accompanies this action.

Peroxide of manganese and oxide of copper have, it is well known, the property of destroying ozone, apparently without limit. The

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authors have found that these oxides undergo no sensible increase of weight, even after the destruction of 50 or 60 milligrammes of ozone. The same oxides, when brought into contact with oxygen contracted by the spark, restore it to nearly its original volume.

Hydrogen gas, purified with care, and perfectly dry, was not changed in volume by the action either of the electrical spark, or of the silent discharge.

A similar negative result was obtained with nitrogen and the silent discharge; but with the spark a very slight alteration of volume appeared to occur, the cause of which is still under investigation.

In the experiments now described, the electrical sparks and discharge were always obtained from the common friction-machine. The discharge from the induction coil, even when passed through two Leyden jars, produces very insignificant ozone effects. The heat which always accompanies this discharge, and its comparatively feeble tension, sufficiently explain its want of energy.

All the results recently obtained by the authors fully confirm the former experiments of one of them*, that in no case is water produced by the destruction of ozone, whether prepared by electrolysis or by the electrical discharge. They reserve any further expression of their views as to the true relations which exist between ozone and oxygen, till they shall have an opportunity of laying the results of this inquiry in a more complete form before the Society.

"Ice Observations." By David Walker, M.D., Surgeon and Naturalist to the Arctic Discovery Expedition.

The contradictory statements of Dr. Sutherland and Dr. Kane, with regard to the saltness of the ice formed from sea-water,—the former maintaining that sea-water ice contains about one-fourth of the salt of the original water; the latter, that if the cold be sufficiently intense, there will be formed from sea-water a fresh and purer element fit for domestic use,—induced the author to take advantage of his position, as naturalist to the expedition now in the northern seas, to reinvestigate the subject.

The changes which he has observed sea-water to undergo in freezing are the following. When the temperature falls below $+28^{\circ}\cdot5$, it becomes covered with a thin pellicle of ice; after some time this pellicle becomes thicker and presents a vertically striated structure, similar to that of the ordinary cakes of sal-ammoniac. As the ice further increases in thickness, it becomes more compact, but the lowest portion still retains the striated structure. On the surface of the ice, saline crystals, designated by the author "efflorescence," soon begin to form, at first few in number and widely separated, but gradually forming into tufts and ultimately covering the whole surface. At first, the increase in thickness of the ice is rapid, but afterwards the rate of growth is much slower and more uniform. The ice formed yields, on being melted, a solution differing in specific gravity according to the temperature at the time of

* Philosophical Transactions for 1856, Part I.

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congelation, its density being less, the lower the temperature at which the process of congelation took place. Although the author's observations extended from +28°·5 to −42°, he was never able to obtain fresh water from sea-ice, the purest specimen being of specific gravity 1·005, and affording abundant evidence of the presence of salts, especially of chloride of sodium, in such quantity as to render it unfit for domestic purposes.

The efflorescence already referred to appeared sooner or later, according to the temperature of the air, but generally commenced when the ice was $\frac{3}{8}$ of an inch thick, and continued to form till the ice attained a thickness of about 9 inches, when, in consequence of the compactness of the frozen mass, it ceased to appear at the surface. The lower the temperature at which the ice was formed, the more abundant was the efflorescence. Direct experiments made by freezing sea-water in a large tub, showed that the unfrozen residuum contained a considerable portion of salts expressed from the ice. The author therefore infers, that after the efflorescence had ceased to form on the surface, the saline particles were precipitated into the unfrozen liquid below. On exposing the residual liquid from which the ice had been separated to a freezing temperature, a second residuum was obtained, containing more salts than the first; and by repeating the process several times, there remained finally a strong solution of brine.

The author endeavoured, by reversing this process, to procure fresh water. He remelted the ice from sea-water and froze it again, repeating the operation several times. Ice was thus obtained, which, when melted, gave water, having a density of from 1·0025 to 1·0020.

A "heavy nip" having occurred in the floe near the ship afforded an opportunity of examining the quality of the ice at different depths. The thickness of the entire mass was 54 inches; the density of the solution obtained by melting successive portions varied from 1·0078 to 1·0050; those near the surface giving a liquid of higher density than the rest. A specimen taken from the centre of the mass was reserved for analysis.

With regard to the "efflorescence," the author states that its appearance was very different according as the temperature was above or below −25°. In the former case, it exhibited a plumose form, with secondary plumes branching off; in the latter, it consisted of fibrous crystals varying from $\frac{1}{4}$ to 2 inches in length. This efflorescence acts an important part in the breaking up of the floe. From the middle of January cracks and lanes occur in the floe, which subsequently become filled with new ice covered as usual with the saline efflorescence and a little snow. When the sun's rays fall upon this incrustation, it melts and forms a thick liquid on the top. This penetrates gradually through the ice and aids greatly in breaking it up. The author supposes that a process of endosmosis and exosmosis is, in fact, established through the body of the ice. A similar, but less powerful, action is produced by the same cause on the mass of the floe itself.

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In the artificial freezing of sea-water, the ice was found to be vertically striated, and often divisible into two or more layers, while the under surface was always marked by fine lines intersecting each other at definite angles. From the bottom of the vessel thin plates of ice formed in the unfrozen liquid. They varied in length from $\frac{1}{2}$ in. to $2\frac{1}{2}$ in., and contained less salt than the ice formed on the top.

To explain the observation of Dr. Kane as to the freshness of ice formed from sea-water under −30°, the author supposes that it may have depended on the freezing of a portion of sea-water which was covered at the time of its congelation with a stratum of fresh water produced by the melting of bergs. On the 12th of April, 1857, whilst lying off Brown's Island, within about 4 miles of a glacier surrounded by bergs, the author observed a layer of fresh water, 2 or 3 inches in depth, floating, like oil, on the surface of the salt water. To this cause he attributes the occasional occurrence of hummocks from the upper portions of which ice perfectly free from salt can be obtained, while on digging deeper into these hummocks, the ice is always found to lose its freshness.

"Inquiries into the Phænomena of Respiration." By Edward Smith, M.D.

The author gives in this communication the result of numerous inquiries into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration,—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day, and from season to season; and 3rd, the influence of some kinds of exertion.

After a description of the apparatus employed by previous observers, he describes his own apparatus and method. This consists of a spirometer to measure the air inspired, capable of registering any number of cubic inches; and an analytical apparatus to abstract the carbonic acid and vapour from the expired air. The former is a small dry gas-meter, of improved manufacture, and the latter consists of—1st, a desiccator of sulphuric acid to absorb the vapour; 2nd, a gutta-percha box, with chambers and cells, containing caustic potash, and offering a superficies of 700 inches, over which the expired air is passed, and by which the carbonic acid is abstracted; and 3rd, a second desiccator to retain the vapour which the expired air had carried off from the potash box. A small mask is worn, so as to prevent any air entering the lungs without first passing through the spirometer, and the increase in the weight of this with the connecting tube and the first desiccator gives the amount of vapour exhaled, whilst the addition to the weight of the potash box and the second desiccator gives the weight of the carbonic acid expired. The balances employed weigh to the $\frac{1}{100}$ of a grain, with 7 lbs. in the pan. By this apparatus the whole of the carbonic acid was abstracted during the act of expiration, and the experiment could be repeated every few minutes, or continued for any number of hours, and be made whilst sleeping and with certain kinds of exertion.

The amount of carbonic acid expired in the twenty-four hours was

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determined by several sets of experiments. Four of these, consisting of eight experiments, were made upon four gentlemen, on the author, Professor Frankland, F.R.S., Dr. Murie, and Mr. Moul, during the eighteen hours of the working day. In two of them, the whole of the carbonic acid was collected, and in two others the experiment was made during ten minutes at the commencement of each hour, and of each hour after the meals. The quantity of carbonic acid varied from an average of 24·274 oz. in the author to 16·43 oz. in Professor Frankland. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and when scarcely awake 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute; and the whole evolved in the six hours of the night at 1950 grains. Hence the total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 oz. The effect of walking at various speeds is then given, with an estimate of the amount of exertion made by different classes of the community, and of the carbon which would be evolved with that exertion.

The author then states the quantity of air inspired in the working day, which varied from 583 cub. in. per minute in himself to 365 cub. in. per minute in Professor Frankland; the rate of respiration, which varied in different seasons as well as in different persons; the depth of inspiration, from 30 cub. in. to 39·5 cub. in.; and the rate of pulsation. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. One-half the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 gr. to 54·7 cub. in. to as 1 gr. to 58 cub. in. The variations in the carbonic acid evolved in the working day gave an average maximum of 10·43, and minimum of 6·74 grains per minute. The quantity increased after a meal and decreased from each meal, so that the minima were nearly the same, and the maxima were the greatest after breakfast and tea.

The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food; to render the quantity nearly uniform throughout the day, with a little increase at the hours when food had usually been taken, and to cause the secretions to become alkaline*.

The variations from day to day were shown to be connected with the relation of waste and supply on the previous day and night, so that with good health, good night's rest, and sufficient food, the amount of respiration was considerable on the following morning, whilst the reverse occurred with the contrary conditions. Hence the quantities were usually large on the Monday. Temperature was an ever-acting cause of variation, and caused a diminution in the carbonic acid as the temperature rose.

* The quantity of air was reduced 30 per cent., that of vapour in the expired air 50 per cent., the rate of respiration was reduced 7 per cent., and of pulsation 6 per cent.

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The effect of season was to cause a diminution of all the respiratory phenomena as the hot season advanced. The maximum state was in spring, and the minimum at the end of summer, with periods of decrease in June and of increase in October. The diminution in the author was 30 per cent. in the quantity of air, 32 per cent. in the rate of respiration, and 17 per cent. in the carbonic acid. The influence of temperature was considered in relation to season, and it was shown that whilst sudden changes of temperature cause immediate variation in the quantity of carbonic acid, a medium degree of temperature, as of 60°, is accompanied by all the variations in the quantity of carbonic acid, and that there is no relation between any given temperature and quantity of carbonic acid at different seasons. Whatever was the degree of temperature, the quantity of carbonic acid, and all other phenomena of respiration, fell from the beginning of June to the beginning of September. The author then described the influence of atmospheric pressure, and stated that neither temperature nor atmospheric pressure accounts for the seasonal changes.

The kinds of exertion which had been investigated were walking and the treadwheel. Walking at two miles per hour induced an exhalation of 18·1 grs. of carbonic acid per minute, and at three miles per hour of 25·83 grs.; whilst the effect of the treadwheel at Coldbath Fields Prison was to increase the quantity to 48 grs. per minute. All these quantities vary with the season, and hence the author recommends the adoption of relative quantities, the comparison being with the state of the system at rest, and apart from the influence of food.

January 27.—Sir Benjamin C. Brodie, Bart., President, in the Chair.

The following communication was read:—

"On the Effect of Pressure on Electric Conductibility in Metallic Wires." In a Letter from M. Elie Wartmann of Geneva, to Major-General Sabine, Treas. and V.P.R.S.

Geneva, January 3rd, 1859.

My dear Sir,—The newspapers having reported that a society of English shareholders intends to lay a second cable for transatlantic telegraphy, you will allow me to give here a brief account of some experiments by which I have succeeded in proving the effect of pressure on electric conductibility in metallic wires.

The method which I have resorted to is the one devised by MM. Christie and Wheatstone, which is called the electrical bridge. The current of a Bunsen's battery of six large cells was divided between the wire to be tested (a very soft copper wire 0·05 of an inch in diameter, and covered with gutta percha) and another conductor; both being connected with a delicate Ruhmkorff's galvanometer, so that the needle remained on the zero point. All contacts were made invariable by solderings.

No sensible effect being determined by the pressure of nine atmo-

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spheres in a piesometer, I made use of a press which enabled me to produce compressions superior to four hundred atmospheres, consequently superior to that which is suffered by an electric conductor immersed in the ocean, at a depth of 12,420 English feet. The wire, besides its coating, was preserved against permanent deformation by two sheets of thick gutta percha, placed between the steel plates which took hold of it.

The experiments have shown—

1°. That a pressure of thirty atmospheres (a number relative to the sensibility of the galvanometer) diminishes the conducting power of a copper wire for electricity.

2°. That the effect increases with the pressure.

3°. That the diminution remains the same for each compression, as long as the latter does not vary.

4°. That the primitive conducting power is exactly restored when the pressure vanishes altogether.

Many interesting results flow from these conclusions, which I propose to examine in a future letter. For the present, permit me to add, that the fact which I have discovered establishes a new connexion between electricity, heat, and light; for it has been demonstrated by M. de Senarmont—

a. That any artificial increase of density in a non-crystallized solid body diminishes, in the direction in which it is exerted, the conducting power of that body for heat.

b. That in homogeneous media which are in a state of artificial molecular equilibrium, the conformation of the thermic ellipsoid, either oblate or prolate, is always corresponding to that of the optic one.

I shall feel much gratified if you deem this communication worthy to be laid before the Royal Society. * *

I remain, &c.,

ELIE WARTMANN.

GEOLOGICAL SOCIETY.

[Continued from p. 382.]

May 4, 1859.—Prof. J. Phillips, President, in the Chair.

The following communications were read:—

1. "On the Ossiferous Cave, called 'Grotta di Maccagnone,' near Palermo." By Dr. H. Falconer, F.R.S., F.G.S.

In a letter, dated Palermo, March 21, 1859, and addressed to Sir C. Lyell, V.P.G.S., Dr. Falconer first states, that from the Caves along the coast between Palermo and Trapani he has lately obtained remains of *Elephas antiquus*, *Hippopotamus Pentlandi*, *H. siculus*, *Sus priscus* (?), *Equus*, *Bos*, *Cervus intermedius* and another species, *Felis*, *Ursus*, and *Canis*, and coprolites of *Hyæna*; but no remains of *Rhinoceros*, nor of *Elephas primigenius*. These additions to the

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previously ascertained fauna of the Cave-period in Sicily may aid in putting it in relation with the Newer Tertiary deposits of Italy.

The author then proceeds to describe the Grotta di Maccagnone, a previously undescribed ossiferous cave, in the Hippurite-limestone, westward of the Bay of Carini (between Palermo and Trapani). In the breccia below its entrance he met with remains of *Hippopotamus* in abundance, and remains of *Elephas antiquus* in the upper deposit of humus within the cave. But some other fossils were discovered under very interesting and somewhat anomalous conditions in this cave. The interior of the cavern is lined with stalagmite; and at a spot on the roof, where this is denuded, Dr. Falconer found a large patch of bone-breccia containing teeth of Ruminants, bits of carbon, shells of several species of *Helix*, and a vast abundance of flint and agate knives of human manufacture. At other places, and wherever the author had the calcareous coating broken by hammers, he found similar remains. At one spot, on breaking the stalagmite, he found against the roof of the cave a thick calcareo-ochreous layer containing abundance of the coprolites of a large *Hyæna*.

Dr. Falconer draws the following inferences from the study of these facts:—1. That the Maccagnone Cave was filled up to the roof within the human period, so that a thick layer of bone-splinters, teeth, land-shells, and human objects was agglutinated to the roof by the infiltration of water holding lime in solution. 2. That the coprolites of a large *Hyæna* were similarly cemented to the roof at the same period. 3. That subsequently, and within the human period, such a great amount of change took place in the physical configuration of the district as to have caused the cave to be washed out and emptied of its contents, excepting the patches of material cemented to the roof and since coated with additional stalagmite.

2. "On the Jurassic Flora." By Baron Achille de Zigno.

In studying the numerous specimens of Jurassic Plants discovered in the Venetian Alps, Sig. de Zigno has found it necessary to pass in revision all the known species derived from the Jurassic strata in different countries. In preparing his large work on the Fossil Plants of the Oolitic Rocks ('Flora fossilis Formationis Oolithicæ'), two parts of which have been published, the author finds, as may be expected, some discrepancies in the published opinions as to the place which the plant-bearing beds of Scania, Richmond (U. S.), India, Australia, and South Africa respectively are entitled to in the geological scale. As the apparent weight of evidence places some of these deposits in other formations than the Jurassic, and as some are still very doubtfully placed, the author omits them from his sources of Jurassic plants.

In the two parts of his work which he has presented to the Society, the author describes the Jurassic *Calamites* (including the *Asterophyllites*), the *Phyllothecæ*, and *Equiseta*. The plates of figures accompanying the foregoing, but not yet described, are recommended by the author to the notice of English paleobotanists,

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as illustrative of interesting but somewhat obscure Ferns; and he particularly requests that search should be made in the Oolites of Yorkshire for specimens of *Pachypteris* with pinnules having a single midrib. Sig. de Zigno supports Sternberg and Bronn in the suggestion that under the term *Equisetites columnaris* authors have confounded two distinct forms; one from Brora and Yorkshire, with thick joints, and illustrated by König; the other being found in the Lias and Trias. Some remarks on the probable relations of *Glossopteris* and *Sagenopteris* follow.

The remains of Ferns in Jurassic beds of the Venetian Alps are numerous, though the species are few. The fructification is often evident; and the epidermis of the fronds can be sometimes separated for microscopical examination. The *Cycadeæ* have more species; and the *Coniferæ* (especially the *Brachyphylla*) are numerous.

3. "On a Group of supposed Reptilian Eggs (*Oolithes Bathonicæ*) from the Great Oolite of Cirencester." By Professor J. Buckman, F.G.S.

The specimen referred to was obtained by Mr. Dalton from the Harebushes quarry near Cirencester, and presents evidence of a compact cluster of eight oval bodies (each about 2 inches long and 1 inch across) in a mass of oolitic rock. These oval bodies being equally rounded at the ends, and in this differing from birds' eggs, the author thinks that they must have been the eggs of a reptile. The egg-shells were very thin, have been here and there puckered by pressure, and are more or less occupied with calc-spar.

4. "On some Sections of the Strata near Oxford." No. I. By Professor Phillips, Pres. G.S.

In this communication Professor Phillips gave the details of sections showing the base and the top of the Great Oolite in the Valley of the Cherwell. This oolite, with sandy layers below and variable argillaceous beds above (capped by the Cornbrash), has been entirely referred to the Great Oolite formation by the Geological Survey, and has been traced through Northamptonshire to the cuttings in the Great Northern Railway near Stamford and Grantham; and continues through Lincolnshire to the Humber. On the north of that river this series is continued by the Oolite of Brough and Cave, and is recognized again in the Millepore-rock at the base of the Gristhorpe Cliffs. Hence it appears that the calcareous shelly beds of Gristhorpe on the Yorkshire coast are still to be assigned, as they were in earlier works, to the Great Oolite group, notwithstanding the fact that they contain a few fossils which in the South of England are prevalent in the Inferior Oolite, together with many the distribution of which is not there limited to one member of the Great or Bath Oolite series.

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May 18, 1859.—Major-Gen. Portlock, Vice-President, in the Chair.

The following communications were read:—

1. "Palichthyologic Notes, No. 12. Remarks on the Nomenclature of the Fishes of the Old Red Sandstone." By Sir P. Egerton, Bart., M.P., F.R.S., F.G.S. &c.

Premising with some remarks on the in many respects unsatisfactory condition of the nomenclature of the fishes of the Old Red Sandstone, the author refers to the late revival, by Dr. Pander, of the discussion as to the priority of Eichwald's name "Asterolepis" over the "Pterichthys" of Agassiz; and, after a detail of the circumstances of the case, Sir Philip states that there is every reason for the retention of the name *Pterichthys* for the "winged fish" discovered at Cromartie by Miller in 1831, introduced by him to the scientific world in 1839, and named *Pterichthys* by Agassiz in 1840.

The author then proceeded to offer some critical remarks on several of the genera and species which Prof. M'Coy has described from the Old Red Sandstone. *Chirolepis velox*, M'Coy, is regarded by him as a good species; but *C. curtus* as identical with *C. Cummingiæ*, and *C. macrocephalus* with *C. Traillii*. *Chiracanthus grandispinus* and *C. pulverulentus* are regarded as good species; but *C. lateralis* is referred to *C. minor*. *Diplacanthus gibbus* and *D. perarmatus* are accepted. The substitution of *Diplopterax* for *Diplopterus* is not considered necessary. *Diplopterus gracilis* appears to be a variety of *D. Agassizii*. The occurrence of *D. macrolepidotus* in Caithness, and the restriction of *D. macrocephalus* to Lethen-bar and Russia, are regarded as a reason for not accepting Prof. M'Coy's view as to the identity of these two forms.

Osteolepis arenatus, stated by Prof. M'Coy to occur at Orkney, has been met with only in the Gamrie by Sir Philip. *O. brevis* is regarded as a good species, though the apparent breadth of the head has probably been misunderstood. Hugh Miller has well figured and described the cranial anatomy of this species in the 'Footprints.' *Triplopterus Pollexfeni* is also considered to be well established generically and specifically. Sir Philip coincides with Prof. M'Coy in classing *Dipterus* with the *Cœlacanthi*, but observes that it is distinct from *Glyptolepis*. *Dipterus* has but one anal fin. *Dipterus brachypygopterus* and *D. macropygopterus* are, in the author's opinion, synonyms; but *D. Valenciennesi* is regarded by him as distinct.

Conchodus is esteemed by the author only a provisional genus.

Sir Philip agrees with M'Coy in separating from the *Holoptychius* the large fishes of the Coal-measures which have received the name *Rhizodus* from Prof. Owen. The latter have an ossified vertebral column. *Holoptychius* has decidedly two dorsal fins. Some good specimens lately obtained at Dura Den prove that *H. Andersoni* and *H. Flemingii* are specifically the same. The determination of *H. princeps* by scales alone is not regarded as satisfactory; but *H. Sedgwickii* is a good species. *Gyroptychius angustus* and *G. diplopteroides* are considered as good species of a new and important genus;

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but Sir Philip refers them to the *Saurodipteridæ*, not to the *Cœlacanthi*. *Platygnathus Jamiesoni*, Ag., is well-founded, as proved by recent discoveries in Dura Den; but the specimen of jaw named *P. paucidens* by Agassiz is assigned to *Asterolepis* by Hugh Miller.

With regard to the *Placodermata* of M'Coy, *Pterichthys* and *Coccosteus* are the types, and *Chelyophorus* is probably a member of the family; but it is still doubtful whether *Asterolepis* and *Heterosteus* belong to it. *Cephalaspis*, *Pteraspis*, and *Auchenaspis* remain for the limited *Cephalaspidæ*.

Pterichthys had certainly one dorsal, and two ventral fins.

Sir Philip remarks that in *Coccosteus* M'Coy and others have mistaken for vertebral centres the thick lower extremities of the neurapophyses; hence the *C. microspondylus* of M'Coy is a misnomer, and what he terms the "dermal bones of the dorsal fin reversed," in his specimen, are the hæmapophyses. Sir Philip thinks that *C. microspondylus* and *C. trigonaspis* must be regarded as synonyms of *C. decipiens*, Ag. *C. pusillus* is quoted as a good species, and probably the same as one subsequently described by H. Miller as *C. minor*.

In a Supplement to this Memoir Sir P. Egerton gives several extracts from unpublished letters by the late Hugh Miller, descriptive of structural characters of the *Coccosteus*. Among these notes is the description of a small well-defined *Coccosteus* which Sir Philip proposes to signalize as *C. Milleri*.

2. "On the Yellow Sandstone of Dura Den and its Fossil Fishes." By the Rev. John Anderson, D.D., F.G.S. &c.

In his geological remarks on Dura Den, the author described the sedimentary strata in the vicinity as consisting of (in ascending order) 1. Grey sandstone, the equivalent of the Carmylie and Forfarshire flagstones, with *Cephalaspis* and *Pterygotus*. 2. The red and mottled beds, such as those of the Carse of Gowrie, and the Clashbennie zone with *Holoptychius nobilissimus*, *Phyllolepis concentricus*, and *Glyptolepis elegans*. 3. Conglomerates, marls, and cornstone, with few and obscure fossils. 4. The Yellow Sandstone, rich in remains of *Holoptychius* and other fishes, and about 300 or 400 feet in thickness. This sandstone is seen to rest unconformably on the middle or Clashbennie series of the Old Red at the northern opening of the Den, and at the southern end is unconformably overlaid by the Carboniferous rocks. It is also exposed beneath the lower coal-series of Cults, the Lomonds, Binnarty and the Cleish Hills. It is seen also in Western Scotland (Renfrewshire and Ayrshire), and also in Berwickshire and elsewhere in the south, with its Pterichthyan and Holoptychian fossils. In the author's opinion it is entirely distinct from the "Yellow Sandstone" of the Irish geologists.

At Dura Den the yellow sandstone in some spots teems with fossil fish, especially in one thin bed. In 1858 a remarkably fine *Holoptychius Andersoni* was met with; and this, with many other specimens, fully bears out Agassiz's conjectures for completing the form and details of the fish where his materials had been insufficient.

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Dr. Anderson also offered some remarks on the *Glyptopomus minor* (Agass.), the specimen of which was obtained from this locality; and he drew attention to two apparently as yet undescribed fishes also from Dura Den.

LXXV. *Intelligence and Miscellaneous Articles.*

NOTE ON THE STRATIFICATION OF THE ELECTRIC LIGHT. BY MM. QUET AND SEGUIN.

As all electric light arises from an effect of tension, we must regard it as certain that the brilliant and obscure bands of striated discharges correspond with different tensions diffused alternately along the gaseous column. The question is, to know what is the cause of these alternate variations of tension.

The hypothesis of interferences, probably tried by many physicists, has acquired new importance from the recent experiments of Grove. But in those cases in which the discharges arrive *in vacuo* by sparks, nothing shows that the discharges corresponding to each pair of sparks are superposed; and it has not been demonstrated that, by suppressing one of the sparks without modifying the conditions of the other, the stratifications are suppressed. On the other hand, amongst the experiments of Gaugain, Plücker, and Gassiot, and those of our preceding note, there are some in which multiple discharges take place without interfering, and produce effects in which it is always possible to discern the part of each current.

If we assume that the contact breaker occasions a series of interruptions and as many induced currents, we may ask how successive and irregular impulses should give rise to visible interferences and to bands widely separated from each other and very fixed in their position, such as are obtained in a cylindrical Geissler's tube, by employing a weak battery and applying the hand upon the tube. Must we also assume that there is a series of impulses producing interferences when an ordinary Leyden jar is discharged through the tube, or when, after having converted the tube into a condenser, we discharge the interior upon the armature, and all without the interposition of any of those ordinary conductors, such as the moistened thread employed by Gassiot, which slacken the discharge and may render it intermittent?

Most physicists appear inclined to attribute the principal part to the resistance of the medium. That the nature of the medium has an influence upon the stratifications, is a circumstance indicated since the original discovery. But it still remains to be ascertained how this resistance causes the alternations of the electric tension. By varying the experiments of Riess upon the electroscopic states of Geissler's tubes, we have only ascertained that these states change with the resistance placed in the course of the induced currents, and that the same effects of tension are obtained by substituting for the vacuum-tube a column of water*. Until we have some perfectly

* It is well known that the resistances modify the aspect of the luminous

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conclusive experiments, we shall describe some new observations which appear to us to be adapted to prepare gradually for the solution of the question.

1. We have endeavoured to obtain effects analogous to the luminous stratifications, by causing induced and ordinary electricity to act upon light and moveable conductors, perceptibly obeying the known laws of electrical influences.

A plate of glass, 2 centims. in breadth and 15 centims. in length, was sprinkled with powdered coke, and the two ends of the induced wire were caused to act upon the two extremities. Under the conditions indicated, the powder is seen to arrange itself all along the plate in transverse lines distinctly separated from each other by intervals of 2 to 3 millims. The appearance of these lines varies, like that of the stratifications, by the influence of an armature applied against the glass, and according as the discharges take place by contact or by sparks. We obtain similar bands with ordinary electricity; and account for the formation by the propagation of the electricity through the conducting powder, and by the mutual repulsion of the particles.

The induction spark is much elongated, and appears perfectly striated by brilliant and obscure points arranged like a necklace, when it is caused to shoot into the air after having shaken the powdered coke.

Transverse stratifications of the same kind occur in the discharge across a smoky flame. At the apex of a flame of turpentine, the brilliant points are sometimes replaced by so many little distinct flames.

2. With the view of understanding the effects of external conductors, we placed flames between the two plates of a condenser, setting the lateral conductors in action likewise. The plates were charged by the electrical machine; the flame sank, became widened and sometimes bifurcate, the lateral conductors attracted it; it was also seen to elongate itself in a dart in two opposite directions. These circumstances agree with the effect produced by the hand brought near a vacuum-tube in which a flood of continuous light is made to pass; a portion of this flood is attracted by the hand against the wall, and another portion becomes concentrated towards the axis and stratified.

We have reproduced, with the electrical machine, a result that we had already observed with the induction apparatus. A cylindrical vacuum-tube being occupied by a flood of continuous light,

phænomenon. We have found that they may even suppress the bands. Thus by isolating one of the electrodes, and placing the other in contact with one end of the induced wire, by the intermediation of a column of water, we may obtain at pleasure bands and a continuous flood of light; by placing one of the electrodes in contact with one end of the wire and drawing the sparks from the other side, with the intervention of a greater or less resistance, we may substitute the continuous flood for the bands of the negative side, of the positive side, or of the whole length of the tube. The changes of tension may account for these effects.

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if it be touched with two fingers, the flood is seen to divide in two near the point touched; the portion emanating from the positive electrode terminates in a slightly convex surface, distinctly marked by a zone more brilliant than the rest. One would say that there was an accumulation of electrical matter at the spot where the external influence has caused a solution of continuity. This zone is a band in course of formation; it may be rendered more or less distinct, and, instead of a single zone, two or more neighbouring bands may be produced.

3. By means of condensers formed by glass walls, furnished with metallic armatures on the outside, and containing between them a stratum of confined air connected with a manometer, we are at present endeavouring to analyse the movements caused in this gaseous stratum by electrical actions; although no spark passes, a movement is perceptible in it, especially at the moment when one of the plates is discharged upon the other externally.

The preceding experiments prove that electrified gases yield to electrical attractions and repulsions, that more or less conductive media, composed of mobile particles, are disposed by these influences in strata in which the particles are alternately dispersed and accumulated, and that this disposition gives rise to differences of tension and to luminous bands. After this it is not much to assume that in a gaseous column the influences of electricity give origin to dilated and condensed strata, which are very thin in an ordinary gas, but thicken in a rarefied gas; that the dilated strata conducting the electricity, the two opposite fluids acquire, from the two sides of the condensed strata which are less conductive, a sufficient tension to traverse these in the form of a discharge and to illuminate them. The effects of external conductors would have their application in this view.—*Comptes Rendus*, February 14, 1859, p. 338.

ANALYSIS OF A NATIVE SULPHATE OF COPPER AND IRON.
BY M. F. PISANI.

This mineral was found in stalactitic, mammillated masses, often of considerable size, in a cave close by a mine of copper pyrites in the interior of Turkey.

It has the colour of ordinary sulphate of copper, especially the fresh fracture. Inside are observed a quantity of small crystals, often lining drusy cavities. It is almost entirely soluble in cold water, leaving a scarcely perceptible residue. By long exposure to air its surface assumes an ochreous tint, the effect of the peroxidation of the iron, which this mineral contains in large quantity.

On analysis it gave the following result:—

		Oxygen.	Proportions.
Oxide of copper.....	15·56	3·14	1
Protoxide of iron	10·98	2·44	
Sulphuric acid	29·90	17·94	3
Water	43·56	38·72	7
	100·00		

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leading to the formula $(FeO\ CuO)SO^3 + 7HO$, which represents common sulphate of iron in which a portion of the iron has been replaced by copper.—*Comptes Rendus*, April 18, 1859.

ON PROF. C. P. SMYTH'S TENERIFFE METEOROLOGICAL OBSERVATIONS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The Teneriffe Astronomical Experiment (Phil. Trans. 1858) somewhat confirms, in p. 513, my suggestion of meteorological observations at six-hourly intervals (Phil. Mag. in June and July 1842, and *post*). Thus in Table I. the sums of 6, 0; 7, 1; &c. hours A.M. and P.M., and quadruple maximum difference from diurnal mean, are—

Hours.	Sea-level.			Guajara.			Alta Vista.		
	Barom.	Dry therm.	Dew-point.	Barom.	Dry therm.	Dew-point.	Barom.	Dry therm.	Dew-point.
h h									
6, 0	−·005	−0°·6	+0°·3	−·029	+0°·2	−2°·5	+·008	+3°·1	+0°·4
7, 1	·000	·0	+ ·2	−·011	+ ·6	+1·9	−·011	+1·1	+ ·8
8, 2	−·004	− ·5	− ·5	−·015	−2·1	−1·6	−·007	−3·4	−3·2
9, 3	−·007	− ·9	+ ·3	−·007	− ·1	+0·1	+·001	−2·1	+2·1
10, 4	−·003	+ ·7	+1·0	+·001	− ·6	·0	−·003	−1·9	−1·4
11, 5	+·007	+1·0	+ ·5	+·016	+1·7	+2·1	+·019	+2·1	+2·7
Quad. max.	·120	13·2	16·0	·232	35·2	39·6	·168	33·6	40·8

exhibiting the nearly periodic form of these variations.

In page 525, column 1, is "5448" *correct*, as this height does not harmonize with the succession of the other heights?

The fact of the summer maximum temperature occurring later in the year as the mountain is descended (see pp. 517–531), induces me to refer to a paper of mine epitomized in the Proceedings of the Royal Society for March 3, 1842, wherein I compared the thermal theory to the tidal one, proposed the mean heat at a given place on a given day to be proportional to the sun's meridian altitude at the same place on a day previous to the one inquired after, which interval I termed "thermal establishment" of the place. Thus the mer. alt. at the solstice would give the heat at the place of observation from 25 to 30 days afterwards, owing to the local causes of perturbation. These perturbing causes, according to Prof. Smyth, seem to diminish as we ascend; and in the free regions of upper air, the maximum would probably then occur on the day of the solstice itself.

S. M. DRACH.

Chelsea, May 16, 1859.

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INDEX TO VOL. XVII.

ACETAL, on the conversion of aldehyde into, 276.
Acetyle, on the peroxide of, 302.
Acids, organic, on the peroxides of the radicals of the, 301.
Air, on the production of vibrations of, in open tubes, 419.
Airy (G. B.) on Mr. Cayley's trigonometrical theorem, and on Prof. Challis's proof that equations have as many roots, &c., 176.
Aldehyde, on the action of chloride of acetyle on, 195; on the action of phosgene gas upon, 430.
Alloys, on the hardness of, 114.
Amarine, on some products of decomposition of, 429.
Amphibian remains from South Africa and Australia, on, 373.
Amylamine, on the action of bisulphide of carbon upon, 368.
Anderson (Rev. J.) on the yellow sandstone of Dura Den, 446.
Andrews (Dr. T.) on the conversion of oxygen into ozone, 435.
Aniline, on the action of bibromide of ethylene upon, 66; on the action of bichloride of carbon on, 131.
Animals, on the composition of, slaughtered as human food, 291.
Apatite, on the formation of, 128.
Arbutine, on the constitution of, 425.
Archer (C.) on the adaptation of the human eye to varying distances, 224.
Atkinson (Dr. E.), chemical notices from foreign journals by, 275, 422.
Atlantic cable, on the, 27.
Ball (J.) on the veined structure of glaciers, 263.
Barometer, on the semidiurnal oscillation of the, 313.
Beckles (S. H.) on fossil foot-prints in the old red sandstone, 77.
Becquerel (E.) on the phosphorescence of gases by the action of electricity, 383.
Belt (T.) on the origin of whirlwinds, 47.
Benzamic acid, on the action of nitrous acid upon, 371.
Bile, on the action of, upon fats, 145.
Binney (E. W.) on the occurrence of liassic deposits near Carlisle, 305.
Bird, on a fossil, 375.
Books, new: — Boole's Treatise on Differential Equations, 430.
Borodine (M.) on hydrobenzamide and amarine, 428.
Bothriceps australis, description of, 373.
Brewster (Sir D.) on the polarization of the light of comets, 311; on the coloured houppes or sectors of Haidinger, 323.
Brodie (B. C.) on the formation of the peroxides of the radicals of the organic acids, 301.
Bromo-arsenious acid, on, 261.
Buckton (G. B.) on the organo-metallic radicals, and on the isolation of mercuric, plumbic, and stannic ethyle, 212.
Buff (Prof.) on the law of electrolytic conduction, 394.
Calcium, on the preparation of, 278.
Callan (Rev. Prof. N. J.) on an induction coil of great power, 332.
Calvert (F. C.) on the hardness of metals and alloys, 114.
Camera obscura, on the, 1.
Caron (H.) on apatite, Wagnerite, and some artificial species of metallic phosphates, 128; on the artificial formation of minerals, 277.
Cayley (A.) on a theorem in spherical trigonometry, 151; on Poinsot's four new regular solids, 123, 209.
Cephalaspis, on a new species of, 150.
Cetacean, on a fossil, 375.
Challis (Rev. Prof. J.) on the central motion of an elastic fluid, 21; on the theory of Tartini's beats, 25; on the direction of the vibrations of a polarized ray, 102; a proof that every equation has as many roots as it has dimensions, 112, 283; on

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a mathematical theory of heat, 202; on the theory of elliptically-polarized light, 285; on the resistance of the luminiferous medium to the motions of planets and comets, 352; on the relation of pressure to density, 401.
Chemical notices from foreign journals, 275, 422.
Chitonidæ, on the Permian, 308.
Chlorous acid, on the preparation of, 422.
Cinchona alkaloids, researches on the, 218.
Clausius (Prof. R.) on the mean length of the paths described by the separate molecules of gaseous bodies on the occurrence of molecular motion; with remarks on the mechanical theory of heat, 81.
Climate, on terrestrial, as influenced by the distribution of land and water, 181.
Coal, on the vegetable structures in, 308.
Coal-formation, on the probable depth of the, under Oxford and Northamptonshire, 381.
Cockle (J.) on the theory of equations of the fifth degree, 356.
Comet, thoughts on the formation of the tail of a, 78.
Comets, note on the polarization of the light of, 311; on the resistance of the luminiferous medium to the motions of, 352.
Copper and iron, on a native sulphate of, 449.
Craters of elevation, on the theory of, 56.
Crocodilus Hastingsiæ, on the dermal armour of, 375.
Curves of the third order, on, 71.
Cynochampsa laniarius, description of, 378.
Dale (Rev. T. P.) on the influence of temperature on the refraction of light, 222.
Dawes (W. R.) on some large solar spots, 152.
Dawson (Dr. J. W.) on fossil plants from the Devonian rocks of Canada, 147; on the vegetable structures in coal, 308.
Debus (Dr. H.) on the action of ammonia on glyoxal, 211.
Density, on the relation of pressure to, 401.
Deville (H. Sainte-Claire) on apatite, Wagnerite, and some artificial species of metallic phosphates, 129; on the artificial formation of minerals, 277.
Diamides, on the, 63.
Dibromallylammonia, on a compound of, and chloride of mercury, 194.
Dicynodon, on new species of, 306, 380.
Dove (Prof.) on the difference presented by the prismatic spectrum of the electric light *in vacuo* at the positive and negative poles, 79; on the stereoscopic representation of print, as seen through Iceland spar, 414; on the application of the stereoscope to distinguish prints from reprints, 415.
Drach (S. M.) on Prof. Smyth's Teneriffe meteorological observations, 450.
Dumas (M.) on the preparation of calcium, 278; on the equivalents of the elements, 423.
Duppa (B. F.) on the action of pentachloride of phosphorus on malic acid, 281.
Earth, on the thickness of the crust of the, 327, 397.
Egerton (Sir P.) on the fishes of the old red sandstone, 445.
Electric conductibility of metallic wires, on the effect of pressure on, 441.
—— current, on vibrations produced by a, 417.
—— light, on the difference presented by the prismatic spectrum of the, *in vacuo* at the positive and negative poles, 79; on the stratification of the, 109, 269, 432, 447.
Electricity, on the rotation of metallic spheres by, 107, 417; new apparatus for observing atmospheric, 312; note as to the relation of common and voltaic, 345; on certain vibrations produced by, 359, 417; on the phosphorescence of gases by the action of, 383.
Electrolytic conduction, on the law of, 394.
Energy, on the conservation of, 250, 347.

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Engelhardt (M.) on the constitution of hydrobenzamide, 429.
Equation, a proof that every, has as many roots as it has dimensions, 112; remarks on, 176, 283.
Equations of the fifth degree, on the theory of, 356.
Equivalents of the elements, on the, 423.
Erman (Prof. A.) on the structure, melting, and crystallization of ice, 405.
Ethyle-compounds, on some new, 225.
Ethylene, on the action of bibromide of, upon aniline, 66.
Excretine, observations on, 145.
Eye, on the adaptation of the human, to varying distances, 224.
Falconer (Dr. H.) on the Grotta di Maccagnone, 442.
Faraday (Prof. M.) on regelation, and on the conservation of force, 162.
Fats, on the action of bile upon, 145.
Felspar of the Canton granite, on, 258.
Field (F.) on Guayacanite, a new mineral species, 232.
Fluid, on the central motion of an elastic, 21.
Fluids, on the thermal effects of compressing, 364.
Forbes (Prof. J. D.) on ice and glaciers, 197; on certain vibrations produced by electricity, 359.
Force, on the conservation of, 166.
Foucault (M. L.) on the methods used to ascertain the figure of optical surfaces, 151.
Frankland (Dr. E.) on sodium-ethyle and potassium-ethyle, 289.
Frapoli (M.) on the conversion of aldehyde into acetal, 276.
Gages (A.) on the study of some metamorphic rocks, 169.
Galesaurus planiceps, description of, 378.
Gases, on molecular motion in, 81; on the phosphorescence of, by the action of electricity, 383.
Gassiot (J. P.) on the stratification of electrical discharges, 432.
Geological Society, proceedings of the, 72, 147, 229, 305, 373, 442.
Geology, on some points in chemical, 148.
Geuther (M.) on sodium-alcohol, 427.
Gilbert (Dr. J. H.) on the composition of animals slaughtered as human food, 291.
Glaciers, on the theory of the veined structure of, 198, 263.
Gladstone (Dr. J. H.) on the influence of temperature on the refraction of light, 222; on the periods and colours of luminous meteors, 386.
Glycol, researches on, 427.
Glycosine, on the constitution of, 211.
Glyoxal, on the action of ammonia on, 211.
Glyoxaline, on the constitution of, 211.
Gold-field of Ballaarat, on the, 149.
Gore (G.) on the rotation of metallic spheres by electricity, 107; on an apparatus for examining the electrical relations of unequally heated mercury and fluid alloys in conducting liquids, 398.
Griess (P.) on new nitrogenous derivatives of the phenyle- and benzoyle-series, 371.
Grove (W. R.) on the reflexion and inflexion of light by incandescent surfaces, 177.
Guayacanite, a new mineral species, on, 232.
Gunpowder, on the nature of the action of fired, 366.
Haidinger's coloured houppes, observations on, 323.
Harnitzki (M.) on the action of phosgene gas on aldehyde, 430.
Harrison (J. Park) on lunar influence on temperature as connected with serenity of the sky, 153.
Haughton (Rev. Prof. S.) on some rocks and minerals from Central India, 16; notes on mineralogy, 258; on the thickness of the earth's crust, 397.
Hearder (J. N.) on the Atlantic cable, 27; on a new form of telegraph cable intended to reduce the effects of inductive action, 334.
Heat, on the mechanical theory of, 81; on a mathematical theory of, 202; on the distribution of, in the diffraction-spectrum, 247.
Heddle (Dr.) on the pseudomorphic minerals found in Scotland, 42.
Hennessy (Prof. H.) on terrestrial climate as influenced by the distribution of land and water, 181.

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Herapath (Dr. W. B.) on the cinchona alkaloids, 218.
Hislopite, analysis of the new mineral, 16.
Hofmann (Dr. A. W.) on the cyanate and sulphocyanide of phenyle, 63; on the action of bibromide of ethylene upon aniline, 66; on the action of bichloride of carbon on aniline, 131; on the phosphorus-bases, 133, 360; on the action of bisulphide of carbon upon triethylphosphine, 136; on the monamines, 138, 368; on the sulphocyanide and cyanate of naphtyle, 304; on phosphoretted ureas, 360.
Hull (E.) on the probable depth of the coal-formation under Oxford and Northamptonshire, 381.
Hunt (T. S.) on some points in chemical geology, 148.
Hunterite, characters and analysis of the new mineral, 18.
Huxley (T. H.) on the *Stagonolepis Robertsoni*, and on the foot-marks in the sandstones of Cummingstone, 75; on a new species of *Cephalaspis*, 150; on a new species of *Dicynodon*, 306; on some amphibian and reptilian remains from South Africa and Australia, 373; on *Rhamphorhynchus Bucklandi*, 374; on a fossil bird and a fossil Cetacean from New Zealand, 375; on the dermal armour of *Crocodilus Hastingsiæ*, 375.
Hydrobenzamide, on the action of iodide of ethyle upon, 428.
Hydrodynamics, on some questions in, 21.
Ice, on the structure, melting, and cystallization of, 91, 197, 405, 437.
Iceland spar, stereoscopic representation of print as seen through, 414.
Induction coil, on an, of great power in proportion to its length, 332.
Iodo-arsenious acid, on, 122.
Jevons (W. S.) on the semidiurnal oscillation of the barometer, 313.
Johnson (R.) on the hardness of metals and alloys, 114.
Joule (J. P.) on some thermo-dynamic properties of solids, 61; on the thermal effects of compressing fluids, 364.
Kirkby (J. W.) on the Permian Chitonidæ, 308.
Knoblauch (Prof.) on the connexion between the structure and the physical properties of wood, 348.
Lava, on the formation of continuous tabular masses of stony, on steep slopes, 56.
Lawes (J. B.) on the composition of the animals fed and slaughtered as human food, 291.
Lies-Bodart (M.) on the preparation of calcium, 278.
Light, on the reflexion and inflexion of, by incandescent surfaces, 177; on the influence of temperature on the refraction of, 222; on the theory of elliptically-polarized, 285.
——, electric, on the stratification of the, 109, 269, 432, 447.
—— of comets, on the polarization of the, 311.
Limpricht (Prof.) on nitrogen determinations, 422.
Liquid, on the thermal effect of drawing out a film of, 61.
Liquids, on a new method of determining the specific gravity of, 254.
Liver, on the alleged sugar-forming function of the, 142.
Lunar influence on temperature, on, 153.
Lyell (Sir C.) on the formation of continuous tabular masses of stony lava on steep slopes, 56.
Malic acid, on the action of pentachloride of phosphorus on, 281.
Marcet (Dr. W.) on the action of bile upon fats; and on excretine, 145.
Mercuric ethyle, 212.
Metallic spheres, on the rotation of, by electricity, 107.
Metals, on the hardness of, 114.
Meteorite, on a remarkable, 424.
Meteors, luminous, on the periods and colours of, 386.
Meyer (M. A.) on a new method of examining and verifying the specific gravity of bodies, 150.
Mica, black, of Canton granite, on, 259.
Micropholis Stowii, description of, 373.
Miller (J.) on the succession of rocks in the Northern Highlands, 72.
Mineralogy, notes on, 258.
Minerals, new, 16; on the pseudomorphic, found in Scotland, 42; on the artificial formation of, 277; on the liquids contained in certain, 279.

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Molecular motion, on the mean length of the paths described by the separate molecules of gaseous bodies on the occurrence of, 81.
Monamines, contributions towards the history of the, 138, 368.
Müller (Dr. J.) on the thermal effects of the solar spectrum, 233.
Murchison (Sir R. I.) on the geology of the North of Scotland, 72.
Murray (Hon. C. A.) on some minerals from Persia, 307.
Naphtyle, on the sulphocyanide and cyanate of, 304.
Nitrogen, on the determination of, 422.
Object-glass, on the construction of a new, 1.
Optical surfaces, on the methods used to ascertain the figure of, 151.
Oudenodon, description of, 380.
Owen (Prof. R.) on some reptilian remains from South Africa, 378.
Ozone, experiments on, 435.
Pavy (Dr. F. W.) on the sugar-forming function of the liver, 142.
Perkin (W. H.) on the action of pentachloride of phosphorus on malic acid, 281.
Petzval (Prof.) on the camera obscura, 1.
Phenyle, on the cyanate and sulphocyanide of, 63; on new nitrogenous derivatives of, 371.
Phosphates, metallic, on some artificial species of, 128.
Phosphorus-bases, on the, 133, 360.
Picramic acid, on the action of nitrous acid upon, 370.
Pisani (F.) on a native sulphate of copper and iron, 449.
Planets, on the resistance of the luminiferous medium to the motions of, 352.
Plants, on fossil, from the Devonian rocks of Canada, 147.
Plumbic ethyle, 215.
Poinsot's four new regular solids, on, 123, 209.
Polarized ray, on the direction of the vibrations of a, 102.
Polyhedra, on Poinsot's four new regular, 123, 209.
Potassium-ethyle, note on, 289.
Pratt (Archdeacon) on the thickness of the crust of the earth, 327.
Pressure, on the relation of, to density, 401.
Print, stereoscopic representation of, as it appears when viewed through double-refracting spar, 414.
Ptychognathus declivis, description of, 379.
Quet (M.) on the stratification of the electric light, 109, 447.
Rammelsberg (Prof.) on the composition of titaniferous iron ores, 231.
Rankine (W. J. M.) on the conservation of energy, 250, 347.
Regelation, remarks on, 162.
Respiration, on the phænomena of, 439.
Rhamphorhynchus Bucklandi, description of, 374.
Rijke (Prof. P. L.) on a new method of producing a vibration of the air in a tube open at both ends, 419.
Robinson (Rev. T. R.) on the stratification of electric light, 269.
Rocks, on the succession of, in the Northern Highlands, 72.
——, metamorphic, on a method of observation applied to the study of some, 169.
Rosales (H.) on the gold-field of Ballaarat, 149.
Royal Society, proceedings of the, 56, 131, 210, 289, 360, 432.
Salmon (Rev. G.) on curves of the third order, 71.
Salter (J. W.) on the fossils of the Lingula flags, 306.
Schiel (M.) on chlorous acid, 422.
Scrope (G. P.) on the formation of volcanic cones and craters, 229.
Seguin (M.) on the stratification of the electric light, 109, 447.
Simmler (M.) on the liquids contained in certain minerals, 279.
Simpson (Dr. M.) on a compound of dibromallylammonia and chloride of mercury, 194; on the action of chloride of acetyle on aldehyde, 195.
Smith (Dr. E.) on the phænomena of respiration, 439.
Snow-crystals, on some remarkable transformations of, 410.
Sodium-alcohol, on some reactions of, 427.
Sodium-ethyle, 225, 289.
Solanine, on the constitution and products of decomposition of, 426.
Solar spots, on some large, 152.

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Solar spectrum, on the thermal effects of the, 233.
Solids, on some thermo-dynamic properties of, 61.
Soundings, remarks on deep-sea, 97.
Sounds, on the production of, in open tubes, 419.
Specific gravity of bodies, new methods of examining and verifying the, 150, 254.
Stagonolepis Robertsoni, on, 75.
Stannic ethyle, 217.
Steam-ship propulsion, on, 310.
Stereoscope, on the application of the, to distinguish prints from reprints, 415.
Strecker (Dr.) on arbutine, 425.
Sulphocarbamic acid, on the production of, 138.
Tait (P. G.) on the conversion of oxygen into ozone, 435.
Tartini's beats, on the theory of, 25.
Tate (T.) on a method of determining the specific gravity of liquids, 254.
Tayler (J. W.) on the veins of tin-ore at Evigtok, Greenland, 307.
Taylor (R.), biographical notice of the late, 53.
Telegraph cable, on a new form of, 334.
Temperature, on lunar influence on, 153.
Thomas (L.) on the nature of the action of fired gunpowder, 366.
Thomson (Prof. W.) on the thermal effect of drawing out a film of liquid, 61; on a new apparatus for observing atmospheric electricity, 312.
Thornton (R.) on the coal found on the Zambesi, 307.
Tin-ore, on the veins of, at Evigtok, near Arksut, Greenland, 307.
Titaniferous iron-ores, on the composition of, 231.
Triethylphosphine, action of bibromide of ethylene upon, 134; action of bisulphide of carbon upon, 136.
Trigonometry, on a theorem in spherical, 151; remarks on, 176.
Trimethylamine, action of bibromide of ethylene upon, 139.
Trowbridge (Prof. W. P.) on deep-sea explorations, 97.
Tyndall (Prof. J.), remarks on ice and glaciers, 91; on vibrations produced by an electric current, 417.
Ureas, on phosphoretted, 360.
Volcanic cones and craters, on the mode of formation of, 229.
Wagnerite, on, 128.
Walker (Dr. D.) on some properties of ice, 437.
Wallace (Dr. W.) on iodo-arsenious acid, 122; on bromo-arsenious acid, 261.
Wanklyn (J. A.) on some new ethyle-compounds, 225; on sodium-alcohol, 427.
Wartmann (Prof.) on the effect of pressure on the electric conductibility in metallic wires, 441.
Waterston (J. J.) on the formation of the tail of the comet, 78; on the relation of common and voltaic electricity, 345.
Whirlwinds, on the origin of, 47.
Wöhler (Prof.) on a remarkable meteorite, 424.
Wood, on the connexion between the structure and the physical properties of, 348.
Wright (Dr. T.) on the inferior oolite of the South of England, 376.
Wurtz (M.) on acetal and glycol, 275, 427; on the conversion of aldehyde into acetal, 276.
Zigno (Baron) on the jurassic flora, 443.
Zwenger (Dr.) on solanine, 426.

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